CARNEGIE INSTITUTION

OF

WASHINGTON

YEAR BOOK

No. 1

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JANUARY, 1903.

WASHINGTON, D. C.
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1903

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OFFICERS

President of the Institution

Daniel C. Gilman

Executive Committee

Daniel C. Gilman, *Chairman*
Charles D. Walcott, *Secretary*
John S. Billings
Abram S. Hewitt
S. Weir Mitchell
Elihu Root
Carroll D. Wright

Finance Committee

Lyman J. Gage
Henry L. Higginson
D. O. Mills

Office of the Institution
1439 K Street, Washington D. C.

Marcus Baker
Assistant Secretary, in Charge

ARTICLES OF INCORPORATION

OF THE

CARNEGIE INSTITUTION OF WASHINGTON.

We, the undersigned, persons of full age, and citizens of the United States, and a majority of whom are citizens of the District of Columbia, being desirous to establish and maintain, in the City of Washington, in the spirit of Washington, an institution for promoting original research in science, literature, and art, do hereby associate ourselves as a body corporate for said purpose, under An Act to establish a code of Law for the District of Columbia approved March third, nineteen hundred and one, sections 599 to 604 inclusive; and we do hereby certify in pursuance of said act as follows:

First: The name or title by which such institution shall be known in law is CARNEGIE INSTITUTION.

Second. The term for which said Institution is organized is perpetual.

Third. The particular business and objects of the Institution are the promotion of study and research, with power:

(*a*) To acquire, hold, and convey real estate and other property necessary for the purposes of the Institution as herein stated, and to establish general and special funds;

(*b*) To conduct, endow, and assist investigation in any department of science, literature, or art, and to this end to cooperate with governments, universities, colleges, technical schools, learned societies, and individuals;

(*c*) To appoint committees of experts to direct special lines of research;

(*d*) To publish and distribute documents;

(*e*) To conduct lectures;

(*f*) To hold meetings;

(*g*) To acquire and maintain a library;

(*h*) And, in general, to do and perform all things necessary to promote the objects of said Institution.

Fourth. That the affairs, funds, and property of the corporation shall be in general charge of a Board of Trustees, the number of whose members for the first year shall be twenty-seven (27), and shall not thereafter exceed thirty except by a three-fourths vote of said Board.

IN TESTIMONY WHEREOF we have hereto set our names and affixed our seals, at the City of Washington, in the District of Columbia, on the fourth day of January, 1902.

JOHN HAY	[SEAL]
EDWARD D. WHITE	[SEAL]
JOHN S. BILLINGS	[SEAL]
DANIEL C. GILMAN	[SEAL]
CHARLES D. WALCOTT	[SEAL]
CARROLL D. WRIGHT	[SEAL]

DISTRICT OF COLUMBIA: *ss:*

Be it remembered that on this 4th day of January, A. D. 1902, before the subscriber personally appeared the above named John Hay, Edward D. White, John S. Billings, Daniel C. Gilman, Charles D. Walcott, and Carroll D. Wright, to me personally known and known to me to be the persons whose names are subscribed to the foregoing instrument of writing, and severally and personally acknowledged the same to be their act and deed for the uses and purposes therein set forth.

Given under my hand and official seal the day and year above written.

WILLIAM MCNEIR,
Notary Public.

[SEAL]

BY LAWS

OF THE

CARNEGIE INSTITUTION OF WASHINGTON

[As amended and adopted November 25, 1902]

1. The officers of the Board of Trustees shall be a Chairman, a Vice Chairman, and a Secretary, all of whom shall be chosen biennially, by ballot.

2. The annual meeting of the Board shall be held in Washington, on the second Tuesday of December, beginning with the year 1903. Other meetings of the Board may be called by the Executive Committee on twenty days' notice to each member of the board, and they shall be called in the same manner by the Chairman of the Board or by the Secretary, on the written request of seven members of the Board.

3. The Trustees, by ballot, shall appoint a President of Carnegie Institution, whose term of office shall be five years. He may or may not be a member of the Board of Trustees.

4. The Trustees, by ballot, shall designate seven trustees as an Executive Committee. Their terms of office shall be three years, and the members shall be reëligible. The Committee shall determine by lot the term of office. Any person elected to fill a vacancy shall be chosen for the unexpired part of his predecessor's term.

5. The fiscal year of the Institution shall be from November 1 to October 31, inclusive.

6. There shall be a Finance Committee consisting of three members of the Board, to be elected by the Board, to hold office until their successors are elected. The duty of such Finance Committee shall be to consider and recommend to the Board of Trustees such measures as it may believe will promote the financial interests of the Institution.

7. The Executive Committee shall, when the Board is not in session and has not given specific directions, have charge of all arrangements for administration, research, and instruction ; designate advisory committees for specific duties ; determine all payments and salaries ; keep a written record of all their transactions and expenditures, and submit to the Board of Trustees at the annual meeting a report,

which shall be published: *Provided*, That no expenditure shall be authorized or made by them except in pursuance of a previous appropriation voted by the Board.

8. The Executive Committee shall make arrangements for the custody of the funds of the Institution; keep an accurate account of all receipts and disbursements, and submit annually to the Trustees a full statement of the finances of the Institution, and a detailed estimate for the expenditures of the succeeding year.

9. In the event of a vacancy in the office of President, or in his absence or inability to perform his duties, they shall devolve upon the Secretary of the Executive Committee, who shall be a member thereof.

10. At least one month before the annual meeting, the Chairman of the Board of Trustees shall appoint an authorized public accountant to audit the accounts of the Institution.

11. The terms of all officers shall continue until their successors are elected.

12. These By Laws may be amended at any meeting of the Board of Trustees by a majority vote of the entire membership of the Board, provided written notice of the proposed amendment has been mailed to each member of the Board thirty days prior to the meeting.

TRUST DEED BY ANDREW CARNEGIE.

CREATING A TRUST FOR THE BENEFIT OF THE CARNEGIE INSTITUTION OF WASHINGTON, D. C.

I, ANDREW CARNEGIE, of New York, having retired from active business, and deeming it to be my duty and one of my highest privileges to administer the wealth which has come to me as a Trustee in behalf of others: and entertaining the confident belief that one of the best means of discharging that trust is by providing funds for improving and extending the opportunities for study and research in our country; and having full confidence in the gentlemen afternamed, who have at my request signified their willingness to carry out the Trust which I have confided to them, THEREFORE I have transferred to these the Trustees of the Carnegie Institution of Washington Ten Millions of Registered Five Per Cent Bonds of the United States Steel Corporation, the names of said Trustees being as follows:—

Ex Officio

THE PRESIDENT OF THE UNITED STATES
THE PRESIDENT OF THE SENATE
THE SPEAKER OF THE HOUSE OF REPRESENTATIVES
THE SECRETARY OF THE SMITHSONIAN INSTITUTION
THE PRESIDENT OF THE NATIONAL ACADEMY OF SCIENCES

JOHN S. BILLINGS,	New York
WILLIAM N. FREW,	Pennsylvania
LYMAN J. GAGE,	Illinois
DANIEL C. GILMAN,	Maryland
JOHN HAY,	District of Columbia
ABRAM S. HEWITT,	New Jersey
HENRY L. HIGGINSON,	Massachusetts
HENRY HITCHCOCK,	Missouri
CHARLES L. HUTCHINSON,	Illinois
WILLIAM LINDSAY,	Kentucky
SETH LOW,	New York
WAYNE MACVEAGH,	Pennsylvania
D. O. MILLS,	New York
S. WEIR MITCHELL,	Pennsylvania
WILLIAM W. MORROW,	California

ELIHU ROOT, ... New York
JOHN C. SPOONER, ... Wisconsin
ANDREW D. WHITE, ... New York
EDWARD D. WHITE, ... Louisiana
CHARLES D. WALCOTT, ... District of Columbia
CARROLL D. WRIGHT, ... District of Columbia

The said gift is to be held in trust for the purposes hereinafter named or referred to, that is to say, for the purpose of applying the interest or annual income to be obtained from the said bonds or from any other securities which may be substituted for the same: for paying all the expenses which may be incurred in the administration of the Trust by the Trustees, including in said expenses the personal expenses which the Trustees may incur in attending meetings or otherwise in carrying out the business of the Trust: AND SECOND, for paying the sums required by the said Trustees to enable them to carry out the purposes hereafter expressed. I hereby confer on the Trustees all the powers and immunities conferred upon Trustees under the law, and without prejudice to this generality the following powers and immunities, viz.: Power to receive and realize the said Bonds, and the principal sums therein contained and the interest thereof, to grant discharges or receipts therefor, to sell the said Bonds, either by public sale or private bargain, at such prices and on such terms as they may deem reasonable, to assign or transfer the same, to sue for payment of the principal sums or interest, to invest the sums which from time to time may be received from the said Bonds on such securities as Trustees are authorized by the law of the State of New York, Pennsylvania, or Massachusetts, to invest Trust Funds,—and also on such other securities as they in the exercise of their own discretion may select, and to alter or vary the investments from time to time as they may think proper;

And I hereby expressly provide and declare that the Trustees shall to no extent and in no way be responsible for the safety of the said Bonds, or for the sums therein contained, or for the securities upon which the proceeds of the said Bonds may be invested, or for any depreciation in the value of the said Bonds or securities, or for the honesty or solvency of those to whom the same may be entrusted, relying, as I do, solely on the belief that the Trustees herein appointed and their successors, shall act honorably;

And I further hereby empower the Trustees to administer any other funds or property which may be donated or bequeathed to them for the purposes of the Trust; and I also empower them to appoint such officers as they may consider necessary for carrying on the business of the Trust, at such salaries or for such remuneration as they may consider proper, and to make such arrangements, and lay down from time to time such rules as to the signature of deeds, transfers, agreements, cheques, receipts, and other writings, as may secure the safe and convenient transaction of the financial business of the Trust. The Committee shall have the fullest power and discretion in dealing with the income of the Trust, and expending it in such manner as they think best fitted to promote the objects set forth in the following clauses:—

The purposes of the Trust are as follows, and the Revenues therefrom are to be devoted thereto:—

It is proposed to found in the city of Washington, an institution which with the co-operation of institutions now or hereafter established, there or elsewhere, shall in the broadest and most liberal manner encourage investigation, research, and discovery—show the application of knowledge to the improvement of mankind, provide such buildings, laboratories, books, and apparatus, as may be needed; and afford instruction of an advanced character to students properly qualified to profit thereby.

Among its aims are these:

1. To promote original research, paying great attention thereto as one of the most important of all departments.

2. To discover the exceptional man in every department of study whenever and wherever found, inside or outside of schools, and enable him to make the work for which he seems specially designed his life work.

3. To increase facilities for higher education.

4. To increase the efficiency of the Universities and other institutions of learning throughout the country, by utilizing and adding to their existing facilities and aiding teachers in the various institutions for experimental and other work, in these institutions as far as advisable.

5. To enable such students as may find Washington the best point for their special studies, to enjoy the advantages of the Museums, Libraries, Laboratories, Observatory, Meteorological, Piscicultural, and Forestry Schools, and kindred institutions of the several departments of the Government.

6. To ensure the prompt publication and distribution of the results of scientific investigation, a field considered highly important.

If in any year the full income of the Trust cannot be usefully expended or devoted to the purposes herein enumerated, the Committee may pay such sums as they think fit into a Reserve Fund, to be ultimately applied to those purposes, or to the construction of such buildings as it may be found necessary to erect in Washington.

The specific objects named are considered most important in our day, but the Trustees shall have full power, by a majority of two-thirds of their number, to modify the conditions and regulations under which the funds may be dispensed, so as to secure that these shall always be applied in the manner best adapted to the changed conditions of the time; provided always that any modifications shall be in accordance with the purposes of the donor, as expressed in the Trust, and that the Revenues be applied to objects kindred to those named,—the chief purpose of the Founder being to secure if possible for the United States of America leadership in the domain of discovery and the utilization of new forces for the benefit of man.

In Witness Whereof, I have subscribed these presents, consisting of what is printed or typewritten on this and the preceding seven pages, on [twenty-eighth] day of [January], Nineteen Hundred and Two, before these witnesses.

ANDREW CARNEGIE.
January 28th, 1902.

Witnesses,
Louise Whitfield Carnegie,
Estelle Whitfield.

REMARKS BY MR. CARNEGIE ON PRESENTING HIS TRUST DEED.

Mr. Chairman and Members of the Board of Trustees:

I beg first to thank you for so promptly and so cordially coming forward to aid me in this work by the acceptance of trusteeship. The President of the United States writes me in a note of congratulation "I congratulate you especially upon the character, the extraordinarily high character, of the trustees." Those are his words. I believe that that estimate has been generally approved throughout the wide boundaries of our country.

May I say to you that my first idea while I dwelt upon the subject during the summer in Scotland was that it might be reserved for me to fulfil one of Washington's dearest wishes—to establish a university in Washington. I gave it careful study when I returned and was forced to the conclusion that if he were with us here today his finely balanced judgment would decide that such, under present conditions, would not be the best use of wealth. It was a tempting point suggested to me by the president of the women's George Washington Memorial Association, that the George Washington Memorial University, founded by Andrew Carnegie, would link my name with Washington. Well, perhaps that might justify such association with Washington, and perhaps it is reserved for some other man in the future to win that unique place; because if we continue to increase in population as we have done it is not an improbability that it may become a wise step to fulfil Washington's wish. But while that may justify the association of any other name with his, which is a matter of doubt, still I am very certain nothing else would. A suggestion that this gift of mine, which has its own field, which has nothing to do with the University, except as an aid to one if it is established, which has a field of its own, that is entitled to the great name of Washington, is one which I never for a moment could consider. If the coming university under the control of the Nation—as Washington suggested a national institution—is to be established, as it may be in the future, I think the name of Washington should be reserved for that and for that alone. Be it our opportunity in our day and generation to do what we can to extend the boundaries of human knowledge by utilizing existing institutions.

This is intended to coöperate with all existing institutions because one of the objections—the most serious one, which I could not overcome when I was desirous to establish a university here to carry out Washington's idea—was this: That it might tend to weaken existing institutions, while my desire was to coöperate with all kindred institutions, and to establish what would be a source of strength to all of them and not of weakness, and therefore I abandoned the idea of a Washington University or anything of a memorial character.

Gentlemen, a university worthy of Washington, or a memorial worthy of Washington, is not one costing a million dollars, or ten million dollars, or twenty million dollars, but of more. When I contemplated a university in Washington in fulfilment of Washington's great wish I set a larger amount than the largest of these. I take it for granted that no one or no association would think of using the revered name of Washington except for a university of first class rank, something greater and better, if I may be allowed to say so, than we have in our land today—and you all know the sums which are now used for our universities.

Gentlemen, your work now begins, your aims are high, you seek to expand known forces, to discover and utilize unknown forces for the benefit of man. Than this there can scarcely be a greater work. I wish you abundant success, and I venture to prophesy that through your efforts, in coöperation with kindred organizations, our country's contributions through research and the higher science in the domain of which we are now so woefully deficient, will compare in the near future not unfavorably with those of any other land.

Again, gentlemen, from my heart, I thank you, and I will now, with your permission read the deed of trust which has been prepared. I may say that the intended officers of this Institution have a letter from my cashier, stating that the notice of the transfer of the bonds will be sent you early in February. They cannot be transferred until the first of the month. They begin to bear interest on the first day of February. Here is the deed of trust. *

There is nothing so important, I think, as the last clause. This clause follows the deed given to the Scotch universities, in the main. When I proposed it to the committee the chairman said he did not know about assuming so much responsibility as a trustee, and sev-

* Printed on pages xi–xiv.

eral gentlemen also suggested that it was too liberal, and threw too much responsibility upon them. Mr. Arthur Balfour was one of these. I replied to him that my experience was that it is not without the greatest difficulty we find men who can legislate for their own generation, and sometimes we are not quite successful even in that; but, I asked, "Have you ever seen, or heard of a body of men wise enough to legislate for the next generation?" He answered "No, I never have"; and "You are quite right; that is the wisest provision I have ever heard of in a trust deed."

I have nothing more to say to you, gentlemen, having already expressed my thanks; but, as I began with doing this, I feel that I should also like to end doing so, and therefore, I thank you again.

MINUTES OF MEETING OF INCORPORATORS OF THE CARNEGIE INSTITUTION OF WASHINGTON.

The meeting of the Incorporators of the Carnegie Institution was held at the office of the Secretary of State, Washington, D. C., January 4, 1902, at 10 o'clock a. m.

Present: Hon. John Hay, Secretary of State, Justice Edward D. White, Dr. Daniel C. Gilman, Dr. John S. Billings, Hon. Carroll D. Wright, and Dr. Charles D. Walcott.

Mr. Hay was chosen Chairman of the meeting, and Mr. Walcott Secretary.

A draft of the proposed Articles of Incorporation was submitted, discussed, and on motion of Mr. White, amended by adding, in the Fourth Article, the words "and shall not thereafter exceed thirty, except by a three-fourths vote of said Board."

The incorporators then signed and acknowledged the Articles of Incorporation.*

The Articles of Incorporation were then taken to the office of the Recorder of Deeds of the District of Columbia by Mr. Marcus Baker, and filed at 11 o'clock a. m. On receipt of notice of the filing of the Articles of Incorporation, Mr. White moved that the incorporators proceed to ballot for trustees. This was done, and the following persons were unanimously elected:

Ex Officio.

THE PRESIDENT OF THE UNITED STATES.
THE PRESIDENT OF THE SENATE.
THE SPEAKER OF THE HOUSE OF REPRESENTATIVES.
THE SECRETARY OF THE SMITHSONIAN INSTITUTION.
THE PRESIDENT OF THE NATIONAL ACADEMY OF SCIENCES.

GROVER CLEVELAND, ... New Jersey.
JOHN S. BILLINGS, ... New York.
WILLIAM N. FREW, ... Pennsylvania.
LYMAN J. GAGE, ... Illinois.
DANIEL C. GILMAN, ... Maryland.
JOHN HAY, ... District of Columbia.

* Printed on pages vii–viii.

ABRAM S. HEWITT, ...	New Jersey.
HENRY L. HIGGINSON, ...	Massachusetts.
HENRY HITCHCOCK, ...	Missouri.
CHARLES L. HUTCHINSON, . . .	Illinois.
WILLIAM LINDSAY, ...	Kentucky.
SETH LOW, ...	New York.
WAYNE MACVEAGH, ...	Pennsylvania.
D. O. MILLS, ...	New York.
S. WEIR MITCHELL, ...	Pennsylvania.
WILLIAM W. MORROW, ...	California.
ELIHU ROOT, ...	New York.
JOHN C. SPOONER, ...	Wisconsin.
CHARLES D. WALCOTT, ...	District of Columbia.
ANDREW D. WHITE, ...	New York.
EDWARD D. WHITE, ...	Louisiana.
CARROLL D. WRIGHT, ...	District of Columbia.

On motion of Dr. Gilman it was voted to ask Mr. Carnegie whether it would be agreeable to him to call a meeting of the Trustees. He subsequently signified his readiness to meet the Trustees in Washington, January 28, 1902, and the Secretary of State invited them to meet in one of the rooms of the State Department.

On motion of Col. Wright it was voted that the Secretary of the meeting notify the Trustees-elect of their election, and of the time and place of meeting, as approved by Mr. Carnegie, and send a copy of the Articles of Incorporation to each of the Trustees.

At 12.30 the incorporators adjourned.

CHAS. D. WALCOTT,
Secretary of Incorporators.

MINUTES OF FIRST MEETING OF BOARD OF TRUSTEES.

[Abstract.]

The Trustees assembled in the Diplomatic Room, Department of State, Washington, D. C., Wednesday, January 29, 1902, at half past two.

They were called to order by Hon. Abram S. Hewitt, who nominated for temporary chairman Hon. John Hay, who was unanimously elected and took the chair.

Mr. Hewitt then nominated Dr. Charles D. Walcott as temporary secretary, and he was unanimously elected.

The Secretary called the roll and the following Trustees responded to their names:

Ex Officio.

WM. P. FRYE, President of the Senate.
D. B. HENDERSON, Speaker of the House of Representatives.
S. P. LANGLEY, Secretary of the Smithsonian Institution.

Active.

JOHN S. BILLINGS	C. L. HUTCHINSON
WILLIAM N. FREW	WILLIAM LINDSAY
LYMAN J. GAGE	WAYNE MACVEAGH
DANIEL C. GILMAN	D. O. MILLS
JOHN HAY	S. WEIR MITCHELL
ABRAM S. HEWITT	WILLIAM W. MORROW
HENRY L. HIGGINSON	ELIHU ROOT
HENRY HITCHCOCK	CHARLES D. WALCOTT

CARROLL D. WRIGHT

Absent: The President of the United States, the President of the National Academy of Sciences, Seth Low, John C. Spooner, Andrew D. White, and Edward D. White.

The Secretary then read the minutes of the meeting of the incorporators and presented the Articles of Incorporation, after which Mr. Andrew Carnegie was introduced by the Chairman, and made the remarks printed on pages xv–xvii.

The following resolution was presented and unanimously adopted:

"In addition to the personal and individual expressions extended to Mr. Carnegie for what he has done for the world today:

"*Resolved*, That the chairman of this meeting be requested to draft a letter addressed to Mr. Carnegie expressing the views of the Trustees concerning this magnificent gift and the purposes for which it is to be applied as set forth in the letter and other documents which have just been read."

The Secretary then read the following letter from the Home Trust Company:

JANUARY 20TH, 1902.

DEAR SIR: I have been instructed by Mr. Carnegie to transfer $10,000,000.00 of United States Steel Corporation 5 per cent bonds to the Trustees of the Carnegie Institution. These bonds will probably be ready for delivery the early part of February, and as soon as they are received from the Transfer Agents I will keep them in a special box in the vaults of the Hudson Trust Company, Hoboken, N. J., subject to the order of the Trustees. Kindly advise if this will be entirely satisfactory to you.

Very truly yours,

R. A. FRANKS.

President,
Trustees of Carnegie Institution,
Washington, D. C.

Attention was called to the vacancy on the Board caused by the declination of Hon. Grover Cleveland, who had not found it possible to accept a place on the Board on account of his health.

An extract from the letter of ex-President Cleveland to Mr. Carnegie was ordered inserted in the minutes as follows:

WESTLAND, PRINCETON, N. J., *Jan. 3, 1902.*

MY DEAR MR. CARNEGIE: I have received your letter, tendering me the great honor of a place among the trustees who are to administer your noble benefaction in the cause of the highest education.

After careful consideration I have determined to ask you to allow me to decline your exceedingly flattering proffer.

I hope it is unnecessary for me to assure you that in reaching this conclusion, I have duly weighed every element that enters into the subject; and have thoroughly convinced myself that I ought not to undertake the important duty involved in your invitation.

Yours very sincerely,

GROVER CLEVELAND.

The Board then balloted for a trustee to fill the vacancy thus arising, and Mr. William E. Dodge, of New York, was unanimously elected.

A proposed code of By Laws was then presented, discussed, amended, and adopted. This code, as still further amended at the meeting of November 25, 1902, is printed on pages ix–x.

Election of officers, in accordance with the By Laws just adopted, was then held with the following result:

Chairman of the Board of Trustees, Abram S. Hewitt.

Vice Chairman of the Board of Trustees, John S. Billings.

Secretary of the Board of Trustees, Charles D. Walcott.

President of Carnegie Institution, Daniel C. Gilman.

At the second session of the Board, held on January 30, 1902, the following members were

Present:

WM. P. FRYE, President of the Senate.

S. P. LANGLEY, Secretary of the Smithsonian Institution.

JOHN S. BILLINGS.	WILLIAM LINDSAY.
WILLIAM N. FREW.	WAYNE MACVEAGH.
LYMAN J. GAGE.	S. WEIR MITCHELL.
DANIEL C. GILMAN.	WILLIAM W. MORROW.
ABRAM S. HEWITT.	ELIHU ROOT.
HENRY L. HIGGINSON.	CHARLES D. WALCOTT.
HENRY HITCHCOCK.	EDWARD D. WHITE.
C. L. HUTCHINSON.	CARROLL D. WRIGHT.

Absent:

THE PRESIDENT OF THE UNITED STATES.

D. B. HENDERSON, Speaker of the House of Representatives.

ALEX. AGASSIZ, President of the National Academy of Sciences.

WILLIAM E. DODGE.	D. O. MILLS.
SETH LOW.	JOHN C. SPOONER.

ANDREW D. WHITE.

The minutes of the previous meeting were read and approved.

Relative to the acceptance of the Trust created by Mr. Carnegie, it was—

Resolved: That the Board of Trustees, acknowledging the generosity of the gift of Mr. Carnegie, in the foundation of the Institution, desire to express the concurrence of the Trustees in the scope and purpose stated in his deed of trust, and hereby formally accept the donation and the responsibilities connected with it.

It was also voted that the resolution just adopted be forwarded to

Secretary Hay, to be by him sent to Mr. Carnegie, with a letter expressing the views of the Trustees on the gift.

Mr. Hay subsequently transmitted the resolution and with it the following letter:

DEPARTMENT OF STATE,
WASHINGTON, *March 7, 1902.*

HON. ANDREW CARNEGIE,
5 West 51st Street, New York City.

SIR: The Trustees of the Carnegie Institution, which you have recently founded in the city of Washington, formally accepted your gift, by the adoption of the appended Resolution.

At the same time they requested me, as the presiding officer at the first meeting of the Board, to convey to you by a letter an expression of their hearty appreciation of your munificence, and also their admiration of the noble purpose and the liberal spirit which distinguish your foundation.

For the advancement of knowledge and the education of youth, there are already in this country many strong institutions, learned societies, universities, government bureaus, libraries and museums. With all of them the Carnegie Institution can coöperate, while it has a field of its own, carefully indicated in your deed of gift, and more fully explained by the remarks which you addressed to the Board.

Every one of those whom you have chosen as Trustees will regard it as a sacred duty and a pleasure, to uphold the lofty ideal that you have set before them, and to impart to those who come afterwards the spirit of confidence and enthusiasm with which the work has begun.

I am, Sir,
Very respectfully yours, JOHN HAY.

Dr. Gilman, the elected President, then addressed the Board, explaining, so far as they were known to him, the circumstances which preceded the incorporation of the Carnegie Institution. His remarks were extemporaneous and intended to acquaint the Board with his attitude and that of the gentlemen with whom, at Mr. Carnegie's request, he had been associated in these arrangements which preceded the meeting of the Board. He expressed his appreciation of the honor conferred upon him by his selection as President of the Institution, and he indicated in broad outlines the probable methods of procedure. At an early day experts in many branches of science will be selected by the Executive Committee to whom all applications for encouragement and aid will be referred. These experts will be requested to add their own suggestions, and pre-

sent their recommendations in writing. Meanwhile, the Executive Committee will gather information in respect to endowments and establishments for promoting science, at home and abroad, in order that this experience may be at the service of the Trustees, and that there may be coöperation, and not conflict, with other institutions in any plans that may be adopted.

After discussing nominations the following named persons were elected members of the Executive Committee:

JOHN S. BILLINGS.	ELIHU ROOT.
DANIEL C. GILMAN.	CHARLES D. WALCOTT.
ABRAM S. HEWITT.	CARROLL D. WRIGHT.
S. WEIR MITCHELL.	

The following resolutions were then considered and adopted:

Resolved: That the Executive Committee is requested to prepare a report upon the work which should be undertaken by the Carnegie Institution in the near future, such report to be submitted to the Board of Trustees at its next meeting, and to be accompanied with estimates for expenditures required.

Resolved: That the Executive Committee, when they shall have formulated plans of the work which should be undertaken by the Carnegie Institution, shall have the same printed and a copy forwarded to each Trustee prior to the annual meeting in November, 1902.

Resolved: That the Executive Committee is requested to consider the question of a proper administration building for the Carnegie Institution, to be located in Washington, including both a proper site and plans for the same.

At the same meeting authority to expend $75,000 was granted by the following resolution:

Resolved: That to defray current expenses of administration, as well as special accounts which may be made during 1902, $75,000, or so much thereof as necessary, is hereby appropriated from the income of the Institution, and is placed at the disposal of the Executive Committee.

Resolved: That the United States Trust Company be, and is hereby, designated as a depository of funds belonging to the Carnegie Institution, and that the President of the Institution, jointly with the Secretary of the Board of Trustees, be, and are hereby, authorized to endorse all checks payable to the order of the Carnegie Institution and make checks or drafts against the funds to the credit of the Institution and the depository above designated.

The following order of business was then adopted for future meetings:

1. Reading of Minutes of last meeting.
2. Report of the Secretary.
3. Reports of Committees:
   (*a*) Executive Committee.
   (*b*) Auditing Committee.
   (*c*) Special Committees.
4. Amendments to By Laws.
5. Election of officers.
6. Unfinished business.
7. New business.
8. Adjournment.

Thereupon, at 12.15 P. M., the meeting adjourned.

CHAS. D. WALCOTT,
Secretary.

MINUTES OF SECOND MEETING OF THE BOARD OF TRUSTEES.

[Abstract.]

The meeting was held in Washington, at the New Willard Hotel, on Tuesday, November 25, 1902, at 10 A. M.

In the absence of the Chairman, Mr. Hewitt, the Vice Chairman, Mr. Billings, occupied the chair.

On the roll being called by the Secretary, the following gentlemen responded to their names:

Ex Officio.

WILLIAM P. FRYE, President of the Senate.

D. B. HENDERSON, Speaker of the House of Representatives.

S. P. LANGLEY, Secretary of the Smithsonian Institution.

ALEX. AGASSIZ, President of the National Academy of Sciences.

Active.

JOHN S. BILLINGS.
WILLIAM N. FREW.
LYMAN J. GAGE.
DANIEL C. GILMAN.
JOHN HAY.
HENRY L. HIGGINSON.
C. L. HUTCHINSON.
WILLIAM LINDSAY.
WAYNE MACVEAGH.
D. O. MILLS.
S. WEIR MITCHELL.
WILLIAM W. MORROW.
ELIHU ROOT.
CHARLES D. WALCOTT.
EDWARD D. WHITE.
CARROLL D. WRIGHT.

Absent:

Ex Officio.

THE PRESIDENT OF THE UNITED STATES.

Active.

WILLIAM E. DODGE.
ABRAM S. HEWITT.
SETH LOW.
JOHN C. SPOONER.
ANDREW D. WHITE.

Letters were received from Messrs. Dodge, Hewitt, Low, and White regretting their inability to be present.

The minutes of the first meeting were read and approved.

The President of the Institution, Mr. Gilman, made a general statement of the work of the Executive Committee and referred to

the report of the Committee, which had been printed and distributed to the Trustees in advance of the meeting.

The Secretary made a brief report, referring principally to the financial transactions of the Institution and submitted the following

Financial Statement:

The following is a statement of receipts and disbursements of the Carnegie Institution of Washington from the beginning, in February to October 31, 1902.

All the preliminary expenses of organization, aggregating $1,825.52, were paid by Mr. Carnegie.

The total receipts are	$250,009.70	
The total disbursements are	30,187.48	
Balance on hand October 31, 1902		$219,822.22
Of this balance there is on deposit with the U. S. Trust Company of New York	$209,822.22	
American Security and Trust Company of Washington	10,000.00	
Total		$219,822.22

The receipts were:

August 1. From the endowment	$250,000.00	
July 3. From interest on deposits in American Security and Trust Company	9.70	
Total		$250,009.70

Consideration of the Executive Committee's report was then taken up, and a long discussion followed on the various recommendations made by the Committee.

At 12.15 p. m. the Board took a recess until 2 p. m.

At the second session there were present:

Ex Officio.

WILLIAM P. FRYE, President of the Senate.
D. B. HENDERSON, Speaker of the House of Representatives.
S. P. LANGLEY, Secretary of the Smithsonian Institution.
ALEX. AGASSIZ, President of the National Academy of Sciences.

Active.

JOHN S. BILLINGS.
WILLIAM N. FREW.
LYMAN J. GAGE.
DANIEL C. GILMAN.
HENRY L. HIGGINSON.
C. L. HUTCHINSON.
WILLIAM LINDSAY.
D. O. MILLS.
S. WEIR MITCHELL.
WILLIAM W. MORROW.
CHARLES D. WALCOTT.
EDWARD D. WHITE.
CARROLL D. WRIGHT.

Absent:

Ex Officio.

THE PRESIDENT OF THE UNITED STATES.

Active.

WILLIAM E. DODGE.
JOHN HAY.
ABRAM S. HEWITT.
SETH LOW.
WAYNE MACVEAGH.
ELIHU ROOT.
JOHN C. SPOONER.
ANDREW D. WHITE.

The Board resumed its discussion of policy and the recommendations of the Executive Committee, especially the purchasing of a site. As the outcome a motion to postpone till the next annual meeting the decision on the question of site was made and carried.

The Board then considered and adopted the following resolution:

Resolved: That from the available income of the Institution $50,000 is hereby appropriated for administrative expenses, $200,000 for grants for research during the fiscal year 1902–'03, $40,000 for a publication fund, the expenditures to be made under the direction of the Executive Committee, and that $100,000 of the available income of the Institution be set apart for a reserve fund during the fiscal year 1902–'03.

Amendments to the By Laws were then considered, and the date of the annual meeting was changed from November to the second Tuesday of December, beginning with the year 1903. By Laws were also adopted providing that the fiscal year of the Institution shall be from November first to October thirty-first, inclusive, and that there shall be a Finance Committee consisting of three members of the Board, to be elected by the Board and to hold office until their successors are elected. The duty of such Finance Committee shall be to consider and recommend to the Board of Trustees such measures as it may believe will promote the financial interests of the Institution.

The By Law in relation to amendments was so modified that thirty days' notice of the proposed amendments must be given prior to the meeting of the Board.

The By Laws as then amended and adopted are printed on pages ix–x.

The Board then proceeded to the choice of the Finance Committee, and elected Messrs. Gage, Mills, and Higginson.

The following minute relative to the death of Mr. Henry Hitchcock was presented by Mr. Higginson and adopted by the Board:

The death of Mr. Henry Hitchcock has deprived this Board of Trustees of a cultured and wise counsellor, a progressive leader, and a valued associate. Mr. Hitchcock stood for all that was noble in manhood and the development of man. His every effort was to serve any cause with which he was connected with all the power and ability he possessed. We tender to the members of his bereaved family sincere sympathy, and place this resolution in our minutes as a permanent record of our appreciation and esteem.

The Board then proceeded to fill the vacancy caused by the death of Mr. Hitchcock. Mr. Ethan Allen Hitchcock was nominated and unanimously elected.

At 4.10 P. M. the Board adjourned.

CHARLES D. WALCOTT,
Secretary.

PROCEEDINGS OF EXECUTIVE COMMITTEE

[Abstract.]

Introduction.—The general powers and duties of the Executive Committee are set forth in the By Laws, pages ix–x ; the special duties and powers assigned it by the action of the Trustees at their meeting on January 30, 1902, are comprised in the five resolutions printed on page xxiv. All the proceedings had by the Committee have been pursuant to and in conformity with those powers and instructions.

Meetings.—The Committee held eight meetings, viz :

First meeting, January 30, 1902, in Washington.
Second meeting, February 8, 1902, in New York City.
Third meeting, March 11, 1902, in New York City.
Fourth meeting, March 25, 1902, in New York City.
Fifth meeting, April 15, 1902, in Washington.
Sixth meeting, October 3 and 4, 1902, in New York City.
Seventh meeting, October 27 and 28, 1902, in Washington.
Eighth meeting, November 14, 1902, in Washington.

Organization.—At its first meeting the Committee organized by electing Mr. Gilman Chairman and Mr. Walcott Secretary. At the same time lots were drawn for the terms of service of members, three to expire with the annual meeting in 1903, two in 1904, and two in 1905. The result of the drawing was as follows :

Terms expiring in December,
1903, Gilman, Mitchell, Wright ;
1904, Billings, Walcott ;
1905, Hewitt, Root.

Seal.—The temporary seal adopted by the Institution contains the words "Carnegie Institution, incorporated January 4, 1902." It is understood that this temporary seal is to be used until a permanent one is adopted by the Trustees.

Advisory Committees.—As soon as it was organized the Executive Committee, in compliance with the instructions of the Trustees, began an investigation to determine what work should be entered upon, in the immediate future, by the Institution. Its first step con-

sisted in the appointment of Advisory Committees. Eighteen such Committees were appointed as follows:

Anthropology:

William H. Holmes, Chief, Bureau of American Ethnology, and Head Curator, Department of Anthropology, U. S. National Museum, Washington, D. C., *Chairman.*

Franz Boas, Curator, Department of Anthropology, American Museum of Natural History, New York, N. Y.

George A. Dorsey, Field Columbian Museum, Chicago, Ill.

Astronomy:

E. C. Pickering, Professor of Astronomy and Director of Harvard Observatory, Cambridge, Mass., *Chairman.*

Lewis Boss, Director of Dudley Observatory, Albany, N. Y.

George E. Hale, Director of Yerkes Observatory, Williams Bay, Wis.

S. P. Langley, Secretary Smithsonian Institution, Washington, D. C.

Simon Newcomb, late Superintendent of Nautical Almanac, Washington, D. C.

Bibliography:

Herbert Putnam, Librarian of Congress, Washington, D. C., *Chairman.*

Cyrus Adler, Librarian, Smithsonian Institution, Washington, D. C.

J. S. Billings, Director New York Public Library, New York, N. Y.

Botany:

Frederick V. Coville, Botanist, Department of Agriculture, Washington, D. C., *Chairman.*

N. L. Britton, Superintendent, New York Botanical Garden, New York, N. Y.

John M. Macfarlane, Professor of Botany, University of Pennsylvania, Philadelphia, Pa.

Gifford Pinchot, Forester, U. S. Department of Agriculture, Washington, D. C.

Chemistry:

Ira Remsen, Professor of Chemistry and President of Johns Hopkins University, Baltimore, Md., *Chairman.*

T. W. Richards, Professor of Chemistry, Harvard University, Cambridge, Mass.

Edgar F. Smith, Professor of Chemistry, University of Pennsylvania, Philadelphia, Pa.

Economics:

Carroll D. Wright, Commissioner of Labor, Washington, D. C., *Chairman.*

Henry W. Farnam, Professor of Political Economy, Yale University, New Haven, Conn.

John B. Clark, Professor of Political Economy, Columbia University, New York, N. Y.

Engineering:

R. H. Thurston, Director of Sibley College, Cornell University, Ithaca, N. Y., *Chairman.*

William H. Burr, Professor of Civil Engineering, Columbia University, New York, N. Y.

George Gibbs, Consulting Engineer, Baldwin Locomotive Works, Philadelphia, Pa.

George S. Morison, Civil Engineer, 49 Wall Street, New York, N. Y.

Charles P. Steinmetz, Electrician, General Electric Co., Schenectady, N. Y.

Geography:

William M. Davis, Professor of Geology, Harvard University, Cambridge, Mass.

Geophysics:

[Joint Committee on Geology and Physics.]

Geology:

T. C. Chamberlin, Head of Geological Department and Director of Museum, University of Chicago, Chicago, Ill., *Chairman.*

Charles R. VanHise, Professor of Geology, University of Wisconsin, Madison, Wis.

Charles D. Walcott, Director of U. S. Geological Survey, Washington, D. C.

History:

J. Franklin Jameson, Head of Department of History, University of Chicago, Chicago, Ill., *Chairman.*

Charles Francis Adams, Boston, Mass.

Andrew C. McLaughlin, Professor of American History, University of Michigan, Ann Arbor, Mich.

Mathematics:

E. H. Moore, Head Professor of Mathematics, University of Chicago, Chicago, Ill., *Chairman.*

Frank Morley, Professor of Mathematics, Johns Hopkins University, Baltimore, Md.

Ormond Stone, Professor of Astronomy and Director of Leander McCormick Observatory, Charlottesville, Va.

Meteorology:

Cleveland Abbe, Professor of Meteorology, U. S. Weather Bureau, Washington, D. C.

Paleontology:

Henry F. Osborn, DaCosta Professor of Zoölogy, Columbia University, New York, N. Y., *Chairman.*

Henry S. Williams, Professor of Geology, Yale University, New Haven, Conn.

Physics:

R. S. Woodward, Dean of School of Pure Science and Professor of Mechanics and Mathematical Physics, Columbia University, New York, N. Y., *Chairman.*

Carl Barus, Professor of Physics, Brown University, Providence, R. I.

A. A. Michelson, Head Professor of Physics, University of Chicago, Chicago, Ill.

Physiology (including Toxicology):

S. Weir Mitchell, Philadelphia, Pa., *Chairman.*

H. P. Bowditch, Professor of Physiology, Harvard Medical School, Cambridge, Mass.

William H. Howell, Dean of Johns Hopkins Medical School, Baltimore, Md.

Psychology:

J. Mark Baldwin, Professor of Psychology, Princeton University, Princeton, N. J.

Zoölogy:

Henry F. Osborn, DaCosta Professor of Zoölogy, Columbia University, New York, N. Y., *Chairman.*

Alex. Agassiz, Curator Natural History Museum, Cambridge, Mass.

W. K. Brooks, Professor of Zoölogy, Johns Hopkins University, Baltimore, Md.

C. Hart Merriam, Chief U. S. Biological Survey, Washington, D. C.

E. B. Wilson, Professor of Zoölogy, Columbia University, New York, N. Y.

These Advisers were requested to give the committee their views on various important suggestions received by the Institution, as well as independent recommendations originating in the committees. The following is a copy of the letter appointing the Advisers and inviting suggestions and recommendations:

MARCH 11, 1902.

DEAR SIR:

The Executive Committee of the Carnegie Institution have been requested by the Trustees to prepare, in the course of the summer, a plan of procedure, and in the meantime to engage in preliminary studies of the problems committed to them, by consultation with acknowledged authorities at home and abroad.

The plan of the Institution includes the appointment from time to time of counsellors, or advisers, to whom the Committee may refer important suggestions, and from whom they may receive independent recommendations. You are invited to act as one of these advisers until the annual meeting of the Trustees, in November next. It is the purpose of the Institution to provide liberally for any expense that may be incurred in clerical service and in travel by those whom they may consult. If it is agreeable to you to accept this invitation, a more personal communication will be addressed to you at an early day. An immediate answer is requested.

Respectfully,

D. C. GILMAN,
President.

The reports received from the Advisory Committees, as far as they relate to scope and plan, are printed in Appendix A.

Circular Letter.

A circular letter was also prepared and sent to nearly a thousand scientific men and investigators of prominence, mainly in the United States. This was accompanied by a pamphlet that included the articles of incorporation, the founder's address, and a list of the officers. The circular letter is as follows:

Letter to the Heads of American Institutions and to Others Interested in the Work of Investigation.

The Carnegie Institution sends to you herewith a copy of Mr. Carnegie's deed of gift and other information in respect to the organization of the new foundation.

Some of the ablest thinkers and investigators in the country have already called attention to important lines of inquiry. Their communications will be referred to special committees in different departments of knowledge—astronomical, physical, chemical, biological, geological, archæological, philological, historical, bibliographical, economical, etc.—and the referees will be requested to add their own suggestions and to report to the Carnegie Institution such methods of procedure and the names of such investigators as they deem likely to advance with wisdom the great purpose of the foundation.

No large appropriations can be made at present, as there will be no income from the fund before August. The summer will be chiefly devoted to a careful study of the problems of scientific investigation, at home and abroad, and in the autumn definite plans of procedure will be formulated.

Any member of the Executive Committee will be glad to receive from you at any time suggestions, opinions, and advice as to fields that the Carnegie Institution ought to occupy and the best methods for carrying forward its work in those fields; but in order that important papers designed for official consideration may be properly recorded and filed, they should be addressed to the President of the Carnegie Institution, 1439 K street, Washington, D. C.

DANIEL C. GILMAN, *Chairman*,
CHARLES D. WALCOTT, *Secretary*,
JOHN S. BILLINGS,
ABRAM S. HEWITT,
S. WEIR MITCHELL,
ELIHU ROOT,
CARROLL D. WRIGHT,
Executive Committee.

MARCH, 1902.

For its guidance, the Committee has formulated and adopted the following statements as to its *Purposes*, *Principles*, *Organization*, and *Policy*:

Purposes.—In connection with the determination of the policy of the Institution, it is necessary to clearly define its *purposes* and to adopt some general plan for organization and administration. The purposes, are declared by the Founder to be

"To found in the city of Washington an institution which, with the coöperation of institutions now or hereafter established, there or

elsewhere, shall in the broadest and most liberal manner encourage investigation, research, and discovery—show the application of knowledge to the improvement of mankind, provide such buildings, laboratories, books, and apparatus as may be needed, and afford instruction of an advanced character to students properly qualified to profit thereby."

And he adds:

"That his chief purpose is to secure, if possible, for the United States of America, leadership in the domain of discovery and the utilization of new forces for the benefit of man."

The trust deed enumerates several aims, all of which may be grouped under two heads, viz:

(A) To promote original research.

(B) To increase facilities for higher education.

Under (A) may be grouped:

(*a*) The promotion of original research "as one of the most important of all subjects."

(*b*) To discover the exceptional man * * * and enable him to make the work for which he seems specially designed his life work.

(*c*) The prompt publication and distribution of the results of scientific investigation.

Under (B) may be grouped:

(*a* and *b*) The increase of facilities for higher education by increasing the efficiency of the universities and other institutions, either by utilizing and adding to their existing facilities or by aiding teachers in various institutions in experimental and other work.

(*c*) To enable such students as may find Washington the best point for their special studies to take advantage of the facilities there for higher education and research.

Principles.—It is the judgment of the Executive Committee that the aims enumerated can be best carried into effect under the following principles, which are to be departed from only in very exceptional cases.

The Institution proposes to undertake—

(A) To promote original research by systematically sustaining—

(*a*) Projects of broad scope that may lead to the discovery and utilization of new forces for the benefit of man, pursuing each with the greatest possible thoroughness.

(*b*) Projects of minor scope that may fill in gaps in knowledge of particular things or restricted fields of research.

(*c*) Administration of a definite or stated research under a single direction by competent individuals.

(*d*) Appointment of Research Assistants.

(B) To increase facilities for higher education by promoting—

(*a*) Original research in universities and institutions of learning by such means as may be practicable and advisable.

(*b*) The use by advanced students of the opportunities offered for special study and research by the Government bureaus in Washington.

The Institution does not proposes to undertake—

(*a*) To do anything that is being well done by other agencies.

(*b*) To do that which can be better done by other agencies.

(*c*) To enter the field of existing organizations that are properly equipped or are likely to be so equipped.

(*d*) To give aid to individuals or other organizations in order to relieve them of financial responsibilities which they are able to carry, or in order that they may divert funds to other purposes.

(*e*) To enter the field of applied science except in unusual cases.

(*f*) To purchase land or erect buildings for any organization.

(*g*) To aid institutions when it is practicable to accomplish the same result by aiding individuals who may or may not be connected with institutions.

(*h*) To provide for a general or liberal course of education.

Organization.—The Executive Committee, keenly realizing the importance of thoroughly investigating and fully considering every proposed action before recommending it to the Trustees, have given much time and thought to the subject of *organization*, and at the several meetings have discussed the suggestions received from individuals and from the Advisory Committees. It is hoped and expected that the Institution will set a high standard for research, This the Committee believes can be best attained and maintained by establishing such laboratories and facilities, not found elsewhere. as are necessary when dealing with great problems.

The Committee is of the opinion that *organization in Washington* should be provided for by—

(*a*) Purchasing in the northwestern suburb of the city a tract of ground suitable for present and future needs.

(*b*) Erecting thereon a central administration building, to serve as the administrative headquarters of research work conducted, directed, or aided by the Carnegie Institution.

(*c*) Establishing such laboratories from time to time as may be deemed advisable.

(*d*) Employing the best qualified men that can be secured for carrying on such research work as it may be decided to undertake in Washington.

(*e*) Continuing and developing the present office organization as the Executive Committee may find it necessary to do in order to properly conduct the work of the Institution.

The only *organization outside of Washington* to be provided for at present should be such advisers and advisory committees as may from time to time be found necessary in connection with the development of the research work of the Institution. It is the opinion of the Committee that such persons and committees should be largely advisory and not executive in their function. Executive work should be in charge of paid employés of the Institution. These may be officers, research associates, and special employés.

Policy.—Soon after the Executive Committee began its investigations it became evident that two lines of policy were open, namely:

(*a*) To sustain broad researches and extended explorations that will greatly add to knowledge.

(*b*) To make small grants.

Research may be defined as original investigation in any field, whether in science, literature, or art. Its limits coincide with the limits of the knowable. In the field of research the function of the Institution should be organization, the substitution of organized for unorganized effort wherever such combination of effort promises the best results; and the prevention, as far as possible, of needless duplication of work. Hitherto, with few exceptions, research has been a matter of individual enterprise, each worker taking up the special problem which chance or taste led him to and treating it in his own way. No investigator, working single handed, can at present approach the largest problems in the broadest way thoroughly and systematically.

With an income large enough to enter upon some large projects and a number of minor ones, it appears to be wiser, at the beginning, to make a number of small grants and to thoroughly prepare to take up some of the larger projects. With this in view the Exec-

utive Committee recommended to the Trustees that there be placed at its disposal for the *fiscal year 1902–'03*, two hundred thousand dollars for aid to special researches in various branches of science, and $40,000 for the publication of the results achieved. During the year plans will be perfected, data secured, and experience gained that will be of great service in formulating recommendations for the ensuing year.

In the opinion of the Committee, the most effective way to discover and develop the exceptional man is to put promising men upon research work under proper guidance and supervision. Those who do not fulfil their promise will soon drop out, and by the survival of the fittest the exceptionally capable man will appear and be given opportunity to accomplish the best that is in him. When the genius is discovered, provide him with the best equipment that can be obtained.

In making grants the wisest policy appears to be to make them to individuals for a specific purpose rather than to institutions for general purposes.

Grants.—Under the authority conferred upon it by the Trustees at their first meeting, the Executive Committee made three grants, as follows:

March 25, 1902.	To the Marine Biological Laboratory, Woods Hole, Mass., for general support	$4,000
April 15, 1902.	To Dr. J. McK. Cattell, Columbia University, New York, for preparing a list of the scientific men of the United States	1,000
April 15, 1902.	To Dr. Hideyo Noguchi and Professor Simon Flexner Philadelphia, Pa., for continuation of their studies of the toxicological actions of snake venom and allied poisons	1,000
Total		$6,000

Since the second meeting of the Trustees, on November 25, 1902, the Executive Committee has made the following grants in the several departments of science mentioned; anthropology, mathematics and other branches will be acted upon later:

Astronomy	$21,000
Bibliography	15,000
Botany	11,700
Chemistry	3,000
Economics	15,000
Engineering	4,500

Exploration	$5,000
Geology	12,000
Geophysics	8,500
History	5,000
Investigation of project for southern and solar observatory	5,000
Investigation of project for physical and geophysical laboratories	5,000
Investigation of natural history projects	5,000
Marine biological research	12,500
Paleontology	1,900
Physics	4,000
Physiology	5,000
Psychology	1,600
Publications	5,500
Research assistants	25,000
Student research work in Washington	10,000
Zoölogy	4,000
Total	$185,200

CHARLES D. WALCOTT,
Secretary.

SUMMARY

As a convenient summary of the plans and methods thus far agreed upon the following minute is approved:

The methods of administration of the Carnegie Institution thus far developed are general rather than specific.

The encouragement of any branch of science comes within the possible scope of this foundation, but as the fund, munificent as it is, is inadequate to meet the requests for aid already presented, not to mention others which are foreseen though not yet formulated, attention has been concentrated upon a selection of those objects which, at this time and in our country, seem to require immediate assistance.

Efforts have been and will be made to secure coöperation with other agencies established for the advancement of knowledge, while care will be exercised to refrain from interference or rivalry with them. Accordingly, ground already occupied will be avoided. For example, if medical research is provided for by other agencies, as it appears to be, the Carnegie Institution will not enter that field. Systematic education, abundantly provided for in this country by universities, colleges, professional schools, and schools of technology, will not be undertaken. Nor will the assistance of meritorious students in the early stages of their studies come within the scope of this foundation. Sites or buildings for other institutions will not be provided.

Specific grants have been and will be made, for definite purposes, to individual investigators, young or old, of marked ability, and for assistance, books, instruments, apparatus, and materials. It is understood that such purchases are the property of the Carnegie Institution and subject to its control. The persons thus aided will be expected to report upon the methods followed and the results obtained. In the publication of results it is expected that the writer will say that he was aided by the Carnegie Institution of Washington, unless it be requested that this fact be not made known.

In order to carry out the Founder's instructions in respect to bringing to Washington highly qualified persons who wish to profit by the opportunities for observation and research afforded by the various scientific bureaus of the United States Government, a certain sum is set apart for this purpose.

In addition, the Carnegie Institution will appoint from time to time a number of persons to be known as Research Assistants, who may or may not reside in Washington, and who shall undertake to carry on such special investigations as may be entrusted to them by the Institution. The appointments will be made for a year, and may be renewed in any case where it seems desirable. Permission may be given to go abroad, if special advantages not accessible in this country can thus be secured.

Publication is regarded by the Founder as of special importance. Accordingly, appropriations will be made for this purpose, especially for the printing of papers of acknowledged importance, so abstruse, so extended, or so costly that without the aid of this fund they may not see the light.

With respect to certain large undertakings involving much expense, which have been or may be suggested, careful preliminary inquiries have been and will be made.

In order to secure the counsel of experts in various departments of knowledge, special advisers have been and will be invited from time to time for consultation. Valuable suggestions and counsel have already been received from such advisers.

DANIEL C. GILMAN,
President of the Carnegie Institution.

WASHINGTON, *November 25, 1902.*

MEMORIAL.

Henry Hitchcock.

1829–1902.

At the annual meeting of the Trustees of the Carnegie Institution held November 25, 1902, the death of Mr. Henry Hitchcock, one of the Trustees, having been announced, it was ordered, on motion of Mr. Higginson, that the following minute be adopted and spread upon the record:

The death of Mr. Henry Hitchcock has deprived this Board of Trustees of a cultured and wise counsellor, a progressive leader, and a valued associate. Mr. Hitchcock stood for all that was noble in manhood and the development of man. His every effort was to serve any cause with which he was connected with all the power and ability he possessed. We tender to the members of his bereaved family sincere sympathy, and place this resolution in our minutes as a permanent record of our appreciation and esteem.

The following account is copied from the Obituary record of graduates of Yale University, June, 1902, No. 61, pp. 135–138:

Henry Hitchcock, son of Hon. Henry Hitchcock (University of Vermont, 1813) and Anne (Erwin) Hitchcock, was born on July 3, 1829, at Spring Hill, six miles from Mobile, Ala. His father was a native of Burlington, Vt., Secretary of the Territory of Alabama, Attorney General and afterward Chief Justice of the State of Alabama, a man of the highest character, beloved throughout the State; and his grandfather, Samuel Hitchcock (Harvard, 1777), who married a daughter of Ethan Allen, was United States District and Circuit Judge, drafted the charter of the University of Vermont, was Secretary of the same from 1790 to 1800, and Trustee from its beginning until his death in 1813. His mother was the daughter of Colonel Andrew Erwin, of Bedford county, Tennessee.

After the death of his father his mother removed with her family first to Kentucky, and then to Nashville, Tenn. There he entered the junior class in the University of Nashville, and received the degree of Bachelor of Arts in November, 1846. Immediately afterward he came to New Haven and joined the class, then in its junior year, in Yale College, and graduated with the honor of an oration.

From August to November, 1848, he was a law student in the office of Hon. Willis Hall, Corporation Counsel of New York city, and was then assistant classical teacher in the Worcester, Mass., High School for a year, after which he returned to Nashville and continued his legal studies in the office of Hon. William F. Cooper, later a Justice of the Supreme Court of Tennessee. In September, 1851, he settled permanently in St. Louis, Mo., was admitted to the bar in October, and began practice. During the year 1852 he was assistant editor of the *St. Louis Intelligencer*, and represented that paper at the National Whig Convention in Baltimore, but afterward devoted himself entirely to the practice of his profession.

In 1872 he formed a partnership with George W. Lubke and John Preston Player, and the firm of Hitchcock, Lubke and Player, thus formed, continued until 1882, when Mr. Lubke was elected a Judge of the Circuit Court, soon after which Mr. Player died.

Mr. Hitchcock then practiced alone for two years, and in 1884 formed a limited partnership with Judge George A. Madill and Hon. Gustavus A. Finkelnburg, which expired in 1890. He continued with the latter until July, 1891, and afterward again practiced alone. He devoted himself especially to equity, corporation and constitutional law.

For over forty years he was deeply interested in Washington University, St. Louis, of which he became a director in 1859, and vice president in 1886. In August, 1867, he helped organize its law department, known as the St. Louis Law School, and for the first twelve years was dean. He was also professor of various departments of law until his retirement, in 1884.

After Lincoln's debate with Douglas on the Kansas-Nebraska question, he joined the Republican party and became an active opponent of slavery. In January, 1861, he was elected a member on the "Unconditional Union" ticket of the Missouri State Convention, which was called by the Secession Legislature to consider the relations of Missouri to the Union, but which disappointed expectations, and deposed both governor and legislature, and for more than two years carried on a provisional state government. He took an active part in its proceedings and attended all its sessions until its final adjournment, on July 1, 1863.

He had earnestly desired active service in the war, and as soon as the Union interests in his own State permitted, he entered the army and was appointed Assistant Adjutant General, U. S. Volunteers, and

from October 1, 1864, to the close of the war served as Judge Advocate on the personal staff of General Sherman. He was with the latter on the "March to the Sea" and in the subsequent campaign through the Carolinas, and carried to Washington the dispatches announcing the "Sherman-Johnston truce." He was brevetted Lieutenant-Colonel, and honorably mustered out of service on June 23, 1865.

After the war he spent four months in European travel. Five years later, owing to the failure of his health, he made a voyage to visit his brother, Ethan Allen Hitchcock, who was then engaged in business in Hongkong, China, and is at present Secretary of the Interior.

In August, 1871, he was one of the delegates who organized at Newport, R. I., the National Civil Service Reform League, and from that date was a member of its Executive Committee. He was one of the fourteen signers of the call which resulted in the formation, in August, 1878, at Saratoga, N. Y., of the American Bar Association, and served several years on standing and special committees, notably on the Committee on the Relief of the United States Supreme Court. He prepared the majority report advocating the plan afterward substantially followed by Congress in creating United States Circuit Courts of Appeal. He was elected President of the Association in 1889.

In 1880 he helped organize the Missouri State Bar Association, of which he was President in 1881.

In April, 1896, he was a delegate from Missouri to the American Conference on International Arbitration, held at Washington, D. C., and took part in its debates, earnestly advocating an international arbitration treaty with England.

He delivered addresses on various subjects of professional and public interest, including the annual address before the New York State Bar Association in January, 1887, on "American State Constitutions," afterwards published in the series called "Questions of the Day;" the annual address before the American Bar Association the same year on "General Corporation Laws;" in March, 1889, an address before the Political Science Association of the University of Michigan, on the "Development of the Constitution as Influenced by Chief Justice Marshall," which, with other lectures by well-known lawyers, was published in a volume entitled "Constitutional Law;" and at the centennial celebration of the organization of the

Supreme Court of the United States, in New York, in February, 1890, an address on "The Exercise of the Powers of the Court," a historical review of the principal decisions on constitutional questions. He received the degree of Doctor of Laws from Yale College in 1874.

Since the establishment of the Missouri Botanical Garden at St. Louis, by bequest of Mr. Henry Shaw, in 1889, he had been Vice-President of the Board of Trustees.

Mr. Hitchcock died at his home in St. Louis, after an illness of several weeks, from heart disease, on March 18, 1902, in his seventy-third year.

He married, on March 5, 1857, Mary, eldest daughter of George Collier, a prominent merchant of St. Louis, and had two sons, who, with their mother, survive.

APPENDIX A

REPORTS OF ADVISORY COMMITTEES

REPORT OF ADVISORY COMMITTEE ON ECONOMICS

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The committee appointed to report upon plans for economic research are of the opinion that, among the numerous topics in Economics, Sociology, and Public Law which might be interesting and useful to study, none are at the present time more promising than those which are suggested by the economic and legislative experience of our states. This experience presents such diversities and the matter to be studied is so vast that it is almost impossible for an isolated investigator to deal thoroughly with even a very limited phase of it. The government offices are obviously not in a position to treat it with the freedom demanded by science. The Carnegie Institution is, therefore, both on account of the funds at its command and on account of its power of enlisting the coöperation of scholars throughout the country, in a position of peculiar advantage with respect to this kind of work, and is able to direct a series of investigations of inestimable value, which, but for its assistance, might not be undertaken for many years.

Among the many topics which fall within this general field, we may specify:

(1) Social legislation of the states, which should be critically examined with reference to its results.

(2) The labor movement.

(3) The industrial development of the states.

(4) State and local taxation and finance.

(5) The state regulation of corporations.

The thorough, scientific presentation of these and other allied topics would constitute a monumental economic history of the United States and occupy a place in economic literature hitherto unfilled.

The committee recommend that an appropriation of $15,000 a year be made provisionally for a period of five years, believing that during that time valuable results can be produced; they further recommend that the committee be authorized to carry forward such parts of the work as may be most advantageously undertaken, and to employ such assistance as may be necessary.

Respectfully submitted by

CARROLL D. WRIGHT, *Chairman*,
HENRY W. FARNAM,
JOHN B. CLARK,
Committee.

NEW YORK, *April 9, 1902*.

REPORT OF ADVISORY COMMITTEE ON BOTANY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Botany have the honor to propose the following

PLAN FOR BOTANICAL RESEARCH.

CONTENTS.

OUTLINE OF PLAN.

I. Relation of Vegetation to Environment in the United States.

A. The function and effect of the forest with regard to atmospheric moisture, precipitation, and run-off, and the converse effect on the forest.

B. The establishment and maintenance of a desert botanical laboratory for the purpose of ascertaining the methods by which plants perform their functions under the extraordinary conditions existing in deserts.

II. Botanical Explorations and Researches in Central America and the West Indies.

A. An exploration of these regions with reference to the composition of the different floras, their relationship to each other and to the climate and geological formations with which each is associated, and their interrelationship with the aboriginal or native peoples, bearing on the past influence of the vegetation on the races of mankind and the probable future development of the various areas.

B. An extensive system of collecting and determining species and recording data, as a means of obtaining the facts necessary to a proper understanding of the problems outlined above, and for other purposes.

C. The establishment and maintenance, in the region, of a botanical station, laboratory, and garden for the solution of problems requiring such equipment.

EXPLANATION OF PLAN.

I. Relation of Vegetation to Environment in the United States.

A. *The function and effect of the forest with regard to atmospheric moisture, precipitation, and run-off, and the converse effect on the forest.*

The relation of forests to atmospheric moisture, precipitation, and run-off is one of the largest economic problems of forestry. It is directly concerned with the water supply for dry and irrigation farming, for water power, and for navigation, and with the control of floods. Although it is the text for continual discussion, we do not yet know whether forests influence rainfall or not, or what is the exact relation of the forest to run-off.

More than seventy thousand square miles of national forest reserves have been created, chiefly with a view to the protection of the water supply in the arid and semi-arid west, without adequate scientific demonstration of their effectiveness. It may be noted also that the flood loss along streams which rise in the Southern Appalachian Mountains, during the last twelve months, was upwards of eighteen million dollars. The lack of precise information on a topic so fundamentally important requires to be remedied, and is capable of remedy. The failure of previous investigations has had much to do with the neglect of a technical knowledge of the nature of the forest and of the differences between forest types. The opportunity to reach results of great value is an admirable one. These results will not be reached, at least for many years, by any agency other than the Carnegie Institution, since no other is capable of coöperating at the same time with all the indispensable members of this inquiry. A suitably conducted study will substantially solve this problem; it will not be undertaken unless by the Institution, and it is thoroughly worthy of investigation along the following general lines:

The investigation should be in charge of a trained forester, assisted by a meteorologist, a hydrographer, a plant physiologist, and an authority on plant distribution, each to be of the first rank. A Greek and Latin scholar, with some knowledge of the general subject under investigation, should be employed during one year to examine and sift the historical evidence for climatic change following deforestation.

A digest of meteorological observations and measurements of run-off already at hand is to be studied in relation to forest types, first in the United States and later abroad (notably in British India and the West Indies), if necessary. Simultaneously the historical study above mentioned should proceed, with special reference to Mediterranean countries and British India. Large coöperation may be expected from the Weather Bureau, the Geological Survey, the Bureau of Forestry, and other government organizations. Field study of forest types and rainfall and forest types and run-off, both on restricted areas, would be required, but such studies should take place usually in regions of sudden changes of forest and climate, and should not restrict the general breadth of the investigation, which should be distinctively continental in character.

B. *The establishment and maintenance of a desert botanical laboratory for the purpose of ascertaining the methods by which plants perform their functions under the extraordinary conditions existing in deserts.*

There should be established at some point in the desert region of the southwestern United States a laboratory for the study of the life history of plants under desert conditions, with special reference to the absorption, storage, and transpiration of water. Although there are many botanical laboratories in the humid portions of the temperate regions, as well as several marine laboratories and tropical laboratories devoted in whole or in part to botanical research, a desert botanical laboratory exists nowhere in the world. Yet the phenomena presented in the adaptations of plants to desert conditions are among the most interesting and significant, from an evolutionary point of view, of any in the whole realm of botany.

The economic ground for the establishment of such a laboratory is the enormous development of population and industries that is bound to take place in our arid region during the next hundred years. The basis of that development is agriculture, both with and without irrigation. At the present time comparatively little is known about the peculiar fundamental processes of plant growth under the unusual conditions surrounding plant life in that region. The investigations proposed are of so general a character, so expensive, and so difficult that no agricultural experiment station has as yet undertaken them, and there is no prospect that any station will do so. When, however, the processes of plant growth in our deserts have been thoroughly investigated and are well understood, the botanists of the agricultural experiment stations in the arid states will be in a position to make a practical application of this knowledge to the special agricultural crops of the region.

It would be necessary to provide a building with equipment of apparatus and a reference library, the whole to be in charge of a resident investigator. It would be desirable also to furnish working quarters for about five additional investigators, some of whom undoubtedly would be the botanists of experiment stations or other botanists giving themselves special training for such positions. As the work of the laboratory progressed it might be found wise to make special grants of money to one or more of these additional investigators. The initial period of maintenance should be not less than five years.

II. Botanical Explorations and Researches in Central America and the West Indies.

A. *An exploration of these regions with reference to the composition of the different floras, their relationship to each other and to the climate and geological formations with which each is associated, and their interrelationship with the aboriginal or native peoples, bearing on the past influence of the vegetation on the races of mankind and the probable future development of the various areas.*

One result of the Spanish-American War has been to open to the botanists of the United States a new field of research, that of tropical botany. The floras of Central America and the West Indies offer an accessible, rich, and comparatively unworked field. At the same time the recent discovery of the manner in which the germs of yellow fever and malaria are transmitted has removed the chief obstacle to the penetration of the white race into the tropics. A revolution in the methods of tropical agriculture is a probable, almost inevitable, result of American influence. This has already occurred in the case of cane sugar, the principal tropical crop with which Americans have had to do, and great changes have occurred in the culture of coffee, rice, and bananas as a result of partial influence from the same source. There is every reason to believe that the coming century will see as great a transformation in tropical agriculture as that which has been brought about in America in the past century in temperate agriculture.

The natural vegetation of any area is a resultant of its climatic, topographic, and geological conditions, and the vegetation of adjacent districts varies as these conditions vary. We know in a general way that the low malarial regions of the Central American coasts are covered with a certain type of dense forest. We know that the interior plateaus and mountains have a wholly different type of forest, or no forest at all, and are often healthy places well suited for

the development of population and industries. We need, however, authoritative and precise information about each of these floral areas, its location, boundaries, characteristic vegetation, the interrelation of its component elements to each other, the characteristic features of the climate with which each flora is associated, and the relation of the vegetation to geological formations.

Finally, each flora should be investigated from the standpoint of its interrelationship with the people who are associated with it geographically. A study of the plants used by any aboriginal race, particularly a prehistoric race, throws great light on its industries, migrations, and civilization. The Central American tropics furnish a wealth of material of this sort which has never been exploited from a critical botanical standpoint.

The geographic botanical investigation here outlined promises results of great significance in ethnologic science, while the clear knowledge of the vegetative covering of the region which is contemplated will be fundamentally useful in the social and industrial development of these areas which is plainly foreshadowed by the commercial necessities of the United States.

B. *An extensive system of collecting and determining species and recording data as a means of obtaining the facts necessary to a proper understanding of the problems outlined above, and for other purposes.*

In addition to the necessity of extensive collecting in order to secure the facts necessary in the geographic researches above described, there are other reasons why it is desirable to increase our knowledge of the plants of the West Indies and Central America. These regions have been partially explored from time to time, but continuous systematic examination of them has not yet been possible, and it is certain that a large number of plant species new to science, and of others about which little is known, occur there. Every botanical collector who visits any part of the region brings back previously unknown forms. The field is therefore a most fertile one for taxonomic research.

Many species whose life history has never been studied occur in the West Indies and Central America, and their investigation would certainly shed much light on questions of development and relationship.

Problems in comparative morphology and anatomy almost without number remain unsolved among species existing only in the American tropics, and the geographic distribution of the species comprising the tropical flora can only be made better known by further exploration.

The New York Botanical Garden, by means of a bequest by the late ex-Chief Justice Charles P. Daly and other funds at its command, has undertaken, under the direction of Prof. L. M. Underwood and Prof. N. L. Britton, the preparation and publication of a Systematic Botany of North America, designed to furnish citations and descriptions of all plant species occurring in North America, including subarctic regions, the West Indies, and Central America and comprising all plants from the simplest to the most complex, from the Myxomycetes to the Compositae. Coöperation with the botanists of other institutions has been arranged for in this work.

The further exploration of the North American tropics would render this work the more complete, and therefore the more valuable. Several trained botanical collectors should be put in the field, the material obtained by them to be used in the preparation of the Systematic Botany. The presence of trained collectors in the region would afford opportunity for securing material of particular species needed by investigators in various laboratories for the solution of problems under study.

The economic features of the investigation would certainly be valuable; the uses of tropical plants are multifarious, and many new applications of them and their products in the arts and manufactures would be likely to ensue; the horticultural aspects of the study would also be important.

C. *The establishment and maintenance, in the region, of a botanical station, laboratory, and garden for the solution of problems requiring such equipment.*

The present status of the morphology and physiology of plants rests chiefly upon observations made on species indigenous to a region about fifteen degrees in width extending across Europe and America in the north temperate zone, together with some information derived from the study of a comparatively few species from other parts of the world, which could be cultivated in the region in question under more or less abnormal conditions.

It has been made evident many times that generalizations reached by such limited and circumscribed methods are unsafe and misleading, and it has become plainly apparent that the entire subject of botany may be developed in a manner commensurate with its scientific and economic importance only when a systematic effort is made to extend investigations to cover the vegetation of the tropics, in which many of the more important physiological and morphological

types are abundant, exhibiting a range of adaptation and a luxuriance of development unknown in the temperate regions.

The desirability and necessity for such an extension of botanical research has long been recognized, yet but little progress has been made in securing adequate facilities in the matter. The Dutch government maintains a research laboratory in pure botany in connection with its great plantations and collections at Buitenzorg, Java, and the small number of botanists who have been able to undertake the long journey to this laboratory have achieved results which fully justify the above estimate of the value of such an institution. The government of India is devoting some attention to similar developments in Ceylon and in one or two other places. An effort was made to organize a tropical laboratory in the West Indies a few years since, but the movement was interrupted by the Spanish-American War, and it will not be possible to resume the plan for organization except by such aid as might be given by the Carnegie Institution.

The establishment of a botanical station of the intended scope and functions would not only afford opportunities for the furtherance of research in all of the strictly technical aspects of the science, but the results obtained would include much of economic importance at a time when it seems necessary for all tropical American countries to improve their methods, or modify the character of their agricultural operations. A station of the above kind would be easily accessible to all botanical investigators in America, and might, if properly located, become the foremost tropical laboratory in the world, since any part of tropical America is also capable of being reached quickly by European students.

The material equipment of the proposed station would consist of a suitable building of metal or stone sufficient to accommodate a scientific staff of two or three persons and provide additional space for at least a dozen investigators. The building should be furnished with the necessary apparatus and appliances for microscopical and experimental research and a small working collection of books. Stress is to be laid on the fact that the best method of management of such stations consists not in acquiring an extensive outfit for the purpose of anticipating all of the wants of the workers who may visit it, but in furnishing the elementary essentials of a station as a beginning and then maintaining the ability to meet the particular needs of the individual investigator. The station should have under its control a few acres of ground in which cultural tests, operations, and

experiments may be carried on, and it should be provided with means to make limited explorations in the adjacent country to secure material needed by its investigators.

A tropical station should reach its highest efficiency when placed under the direction of a competent botanist, who should become a resident in the vicinity of the laboratory. The director should have as an assistant a person trained in the methods of laboratory practice and whose most important duty would consist in securing living material for the investigators who might visit the station. The proper maintenance of the building would require the services of a keeper or superintendent who should control the work of the laborers and gardeners in the care of the building.

The laboratory should be organized for a period of five years. With the information at hand accumulated by the former tropical laboratory commission, the director could proceed at once to the selection of a site and the erection of a suitable building. This work and the arrangement of the equipment would consume the greater part of a year, during which period the director only need be employed, the remainder of the staff beginning their duties at the close of the first year. The assistant to the director might, however, profitably begin his duties at any time convenient in the establishment of the station.

ESTIMATE OF COST.

Relation of Forests to Climate.

Forester in charge	$1,500	
Traveling and other expenses	1,500	
		$3,000
Historical study, one year only		1,500
Collaborating meteorologist—report, research, and advice		1,000
Collaborating hydrographer—report, research, and advice		1,000
Collaboration in plant physiology and geographic distribution		1,000
		$7,500
First year, $7,500	$7,500	
Four subsequent years at $6,000	24,000	
Total for five years	$31,500	

Desert Laboratory.

Equipment:		
Building	$2,000	
Apparatus	2,000	
Books	1,000	
		$5,000

Maintenance for five years:

First year	$3,000	
Second year	3,250	
Third year	3,500	
Fourth year	3,750	
Fifth year	4,000	
		$17,500
Total for five years		$22,500

TROPICAL PLANT GEOGRAPHY.

Working force:		
Botanist (six months)	$1,500	
Botanical assistants (three)	4,200	
Geological assistant	2,000	
Bibliographical assistant	720	
Photographer	1,000	
		$9,420
Traveling expenses		4,000
Supplies and miscellaneous expenses		2,580
Total for the year		$16,000

COLLECTING TROPICAL PLANTS.

Salaries of three experienced tropical collectors for six months, at $125 per month each	$2,250
Expenses of these collectors at $150 per month each	2,700
Contingent fund	50
Total for the year	$5,000

TROPICAL LABORATORY.

Expenditures during the first year of organization:		
Building	$3,000	
Equipment	2,000	
Salary of director	2,500	
Expenses of director in selecting site	500	
		$8,000
Maintenance during second year:		
Salary of director	$3,000	
Salary of assistant	1,500	
Salary of keeper	1,000	
Gardeners and laborers	1,000	
Additions to equipment, repairs, etc	1,000	
		$7,500
Maintenance during third year		8,000
Maintenance during fourth year		8,000
Maintenance during fifth year		8,000
Total cost of establishment and maintenance of tropical laboratory and station during five years		$39,500

SUMMARY OF FIRST YEAR'S COST.

Relation of Forests to Climate	$7,500
Desert Laboratory	8,000
Tropical Plant Geography	16,000
Collecting Tropical Plants	5,000
Tropical Laboratory	8,000
Total	$44,500

ESTIMATED YEARLY AVERAGE FOR FIVE YEARS.

(Each investigation should be continued for not less than five years.)

Relation of Forests to Climate	$6,300
Desert Laboratory	4,500
Tropical Plant Geography	16,000
Collecting Tropical Plants	5,000
Tropical Laboratory	7,900
Total yearly average	$39,700

These estimates do not include the publication of results.

* * * * * * *

Respectfully submitted by

FREDERICK V. COVILLE, *Chairman*,
N. L. BRITTON,
JOHN M. MACFARLANE,
GIFFORD PINCHOT,
Committee.

WASHINGTON, *June 28, 1902*.

REPORT OF ADVISORY COMMITTEE ON PHYSICS

CONTENTS.

To the Executive Committee of the Carnegie Institution:

GENTLEMEN:- Your Committee appointed to suggest ways and means for the increase and utilization of knowledge in the domain of Physics beg to submit the following report:

INTRODUCTION.

By common consent the domain of Physics is divided into several fields, which, though overlapping to some extent, are sufficiently distinct to justify separate consideration. These fields are those of Heat, Light, Electricity, Sound, and Mechanics, by which latter all of the others are to a greater or less extent correlated.

Great advances have been made in recent times, especially during the last half century, in all of these subjects. One needs only to recall the astonishing progress in the practical applications of physi-

cal science in our own time to realize this fact. The developments of heat engines, photography, electricity, telephony, telegraphy, etc., familiar to everybody, afford sufficient justification if any is needed from the utilitarian point of view, for the promotion of physical research. But surprising and gratifying as have been these advances, and well worked as have been all of the fields referred to, there is the amplest room for improvement in every one of them and promise of still more important practical developments from investigations in most of them.

In the past, many capital discoveries and many wide generalizations have been made by isolated workers, mostly college or university professors, who have pursued research more or less incidentally in connection with their duties of instruction. Often such discoveries have required only meagre instrumental equipment and mediocre intellectual attainments, for the phenomena to be observed lay close at hand. With the advance of science, however, it is becoming continually essential to attack larger and more difficult problems. In general, the more recondite the phenomena to be investigated the more elaborate must be both the instrumental and the intellectual equipment of the investigator; and the more essential is it that he should devote his entire time and energy, rather than a fraction of them, to his work of research. There are some physical questions, also, like those common to astronomy and geodesy, which require for their solution the cooperation of a number of experts through a series of years. Such cooperation in the great national and private observatories and in national and international geodetic bureaus has hitherto secured results of immense theoretical and practical importance. Similarly, the Royal Institution of London has been made famous by the researches of Davy, Faraday, Tyndall, and Dewar.

It is clear, then, that there are at the present epoch unlimited opportunities for physical research; and that we may confidentially expect a rich harvest of results from almost any field diligently cultivated by one or more competent investigators. In conformity with this view, your Committee is of opinion that the Carnegie Institution by promoting research in the more refined and difficult problems of physics may not only advance knowledge directly through important resulting discoveries, but may also advance knowledge indirectly by so distinct a recognition of the value of abstract investigation.

Your Committee believe it best to consider all privileges and appointments as emanating from the Institution and none as being obtainable on solicitation from without. Vacancies which may be filled by the best applicant who happens to present himself, should not exist. The full justification for such honors as may be conferred, should be found in the testimony of the annals of physical science. In recommending the following course of action, the Committee, therefore, beg leave to state at the outset, that it is not their design to have all the privileges to be hereinafter mentioned, conferred either continuously or at once; that it would rather be their policy to find the physicists first, and then to build the facilities around them. Furthermore, they have, throughout their work, proceeded from the conviction that as it is the purpose of a university to develop the untried investigator, so it should be the function of the Carnegie Institution to begin with the physicist whose development is acknowledged; that it should extend to him its unstinted opportunities, in proportion as his powers of research are keener and more mature.

The following specific recommendations for effectively encouraging research in pure physics in the United States, are given first in outline, secondly in detail, and supplemented by a provisional plan.

II. Recommendations in Outline.

The recommendations of your Committee are:

(1) *Laboratory.*—To establish a well equipped physical laboratory, to be devoted to research in pure physics, exclusively, and particularly to investigations which, from their character, from the lack of apparatus necessary, etc., can not be advantageeusly done elsewhere. The completion of the laboratory is to be approached gradually by appropriating a definite fund each year, which fund shall be used as a minimum permanent endowment forever after. The resources which so accumulate will in due time suffice for the construction of buildings, for equipment, salaries and the general maintenance of the said laboratory, in the way hereinafter to be more definitely specified. An annuity of 150,000 dollars is estimated as sufficient for this purpose.

(2) *Temporary Associates.*—To secure the co-operation with the Carnegie Institution, of physicists of recognized eminence, who are already provided with working facilities at other institutions of learning or elsewhere, by an annual grant for a specified term of years, in recognition of their continuous contributions to the scien-

tific transactions of the Carnegie Institution. Twenty grants of 2000 dollars each, to be designated ***temporary non-resident research associateships***, or a total of 40,000 dollars per annum, is deemed sufficient for this purpose.

(3) *Permanent Associates.* To establish a few small laboratories in different parts of the country, placed in charge of persons specially eminent for their exceptional experimental skill in the direction of discovery in pure physics and who will not profit by the recommendations under (1) and (2). The advance of physics depends so much on the intellectual effort of individuals, many of whom can not do their best work under corporate surroundings, that this recognition of the habits of work of persons whose scientific record is beyond question, has seemed to the Committee a provision of importance. Frequently, moreover, the character of the work will demand that it be done in special localities. The physicists here in question are not to lose financially by being transferred to the Carnegie Institution; consesequently the following estimates of the grants, to be known as *permanent non-resident research associateships* are given as an average case; viz., for laboratory and grounds, 10,000 dollars; for salary, 6,000 dollars; for running expenses, equipment and assistants, 4,000 dollars. The Committee recommend that two such private laboratories be open for establishment at once, to be increased to four in the course of time, as the funds become available. The total annual expenditure is not to exceed 40,000 dollars.

(4) *Grants.* To establish a fund for grants given to persons of recognized ability, or to societies, for apparatus, laboratory equipment, clerking, shopwork, publication, etc. The Committee have learned that this need has already been partially met by other endowments, and that a wiser provision for the material status of the individual investigator is the more important demand at the present time. They therefore deem an annuity of 10,000 dollars sufficient, under existing circumstances, for grants of the kind specified.

(5) *Council.* To retain a Council of not less than three nor more than five physicists of the highest eminence, whose duty it shall be to inaugurate the research facilities specified and to watch over their progress. A fee of 2,000 dollars per annum to each member is deemed sufficient for the services asked.

(6) *Summarv of Annuities.* The total annual appropriation recommended by your Committee for research in pure physics stands, therefore, as follows :

Annuity for a physical laboratory, etc.,	$150,000
for permanent research associates with small laboratories,	40,000
for temporary research associates,	40,000
for grants of apparatus, etc.,	10,000
for the Council,	10,000
Total,	$250,000

III. Recommendations in Detail.

The Committee further beg leave to submit the following more detailed outline of the laboratory, its government, personnel, functions, equipment and maintenance, together with a final table in which the appropriation recommended is differently itemized.

(A) Laboratory, Resident Officers and Council.

(7) *Laboratory.* The Council proposes that a laboratory, devoted to the higher order of physical research, be built in some specially favorable locality. The work to be prosecuted in this laboratory is to be purely investigational in the broadest sense, to the exclusion of mere routine work of any kind.

The cost of the laboratory is estimated at $400,000; of the grounds, whose location shall be sufficiently isolated to meet the requirements of modern research, at $50,000; of the Power house, shop, etc., at $50,000; making a total of $500,000; the equipment is estimated at $100,000, and the running expenses at $30,000 per annum, or an average of 5,000 dollars for each of the departments hereinafter specified.

(8) *Physicists in charge.* The "physicists in charge" of the work are to be, without exception, persons of internationally acknowledged eminence in physics, and whose records are accessible in the annals of science. No other voucher shall be considered. If the person needed is not to be found in the United States, he may be appointed from abroad, but no position need be filled continuously.

The salaries of the physicists in charge shall be a minimum of $6,000 per annum. If called from other institutions, they shall be appointed at a salary not less than that received immediately prior to their appointment.

They are to be appointed by the Trustees, upon recommendation of the Council on physics. The recommendation is to be made with

the scientific history of the candidate attached, and the appointment is to be for life, subject to the approval of the Council.

After the age of 62 years, or in case of disability, the physicist in charge is at liberty to become inactive and to ask for a pension.

The physicists in charge shall be appointed as follows:

(1) A physicist in charge of the department of mechanics.

(2) A physicist in charge of the department of experimental molecular physics, including elastics, capillarity, cohesion, diffusion, solution, crystallization, hydromechanics, viscosity, etc.

(3) A physicist in charge of the department of heat, including thermodynamics, thermal analytics, kinetic theory, etc.

(4) A physicist in charge of the department of sound.

(5) A physicist in charge of the department of light and radiation, including geometric optics, spectrum analysis, photography, interference, diffraction, polarization, absorption, dispersion, etc.

(6) A physicist in charge of the department of electricity, including electrostatics, magnetism, electromagnetism, electric current, electrolysis, thermoelectrics, induction, electric oscillation, electro-radiation, etc.

Each department is to be filled only when the efficient man has been found. It need not be filled continuously.

The physicist in charge shall be the controlling officer in each department, and the *resident research associates* and *research assistants* hereinafter to be specified shall be subject to his jurisdiction.

It shall not be necessary for the physicist in charge to devote himself exclusively to the subjects of his department, but a wise and watchful provision for its broadest interests is to be his paramount duty.

(9) *Director.* From among these chiefs of division, one shall be selected by the Council, as Director, who shall have the control of all immediate questions of laboratory government.

Appeals from the decision of the Director may be referred to the Council.

The total salary of the Director shall be not less than $8,000 per annum.

(10) *Executive Officer.* To relieve the Director of the burden of administrative work, there shall be appointed an executive officer subject to the control of the Director, at a salary of not less than $4,000 per

annum. It shall be the primary function of this officer to attend to the financial and other administrative work connected with the laboratory. He is to be a man of scientific attainments and to be nominated by the Director. He shall not be eligible to the Council. This executive officer shall have three or more assistants: as, for instance, an electrician, a foreman, laborers, etc., at an aggregate salary of $5,000, to be appointed by himself.

The executive officer shall assist the Council, whenever called upon, in its administrative work.

(11) *Council.* The general supervision of the work, and the decision on all points beyond the jurisdiction of the Director and vitally controlling the progress of the laboratory shall rest with the Council on physics. This Council of not less than three shall be appointed by the Trustees, and shall consist of persons of the same international distinction already specified. They are to be scientists living in the atmosphere of physical research, acquainted with and fruitfully contributing to contemporaneous scientific investigation in physics.

The members of the Council shall be appointed for a period of six years, after which they may be reappointed. The candidates for non-resident councillorships shall be the non-resident research associates hereinafter specified.

One Councillor shall be nominated every two years, beginning with 1904, by the following institutions or societies and in the order specified: the nomination in 1904 is to be by the physicists in charge and the permanent non-resident associates of the Carnegie Institution; the nomination in 1906 by the pure physicists of the National Academy of Sciences of the United States; the nomination in 1908 by the Physical Society of America; the nomination in 1910 by the physicists in charge and permanent non-resident associates of the Carnegie Institution; and so on in rotation.

The Council shall be empowered to increase its number by nominating not more than two (2) physicists of the grade of eminence specified, and each such appointment shall be for a period of six (6) years.

Vacancies are to be filled by the institution or society which nominated the councillor whose place has become vacant.

But one of the Council shall be a resident of the Laboratory; preferably the Director.

The salary of the members of the Council not otherwise connected with the laboratory shall be $2,000 per annum, together with necessary travelling and clerical expenses.

The non-resident Councillors shall be citizens of the United States, and shall not come from the same institution of learning.

The Council, through the executive officer, shall solicit a library of reprints of the original work done in pure physics, in the United States or elsewhere, by Americans, in order to remain additionally informed as to the status of the physicists of the country, in regard to their efficiency as investigators.

After continuous service of more than twelve (12) years, a member of the Council who has passed sixty-two (62) years of age may become inactive and ask for a pension of his full salary of $2,000.

Three members of the Council shall constitute a quorum, and a majority of votes shall decide.

(B) Resident and Non-resident Associates and Assistants.

(12) *Permanent non-resident associates.*—In recognition of the peculiar habits of work of great investigators, and for other reasons, there shall be established not more than four *permanent non-resident associateships of research.* They shall be recommended to the Trustees by the Council. A specific account of the accomplishments of the candidate is to be attached to the recommendation.

These permanent non-resident associateships shall be granted to persons of the recognized exceptional eminence, herein already specified, in recognition of their past and present continuous services to scientific research in physics. If appointed from another institution, it shall be at a salary not less than that received immediately prior to the appointment and not less than 6,000 dollars per annum. They shall be provided with a laboratory of their own design, costing not more than $10,000 for grounds and building, and not more than $4,000 per annum for assistants, equipment and maintenance.

These associateships shall be filled only when the person of recognized ability has been found, and they need not be filled continuously.

(13) *Temporary non-resident associates.*—To bring the laboratory in touch with the wishes of the Founder as expressed in his deed of trust; to provide that the Institution may react usefully on the country at large, with a view to stimulating research; and in order to reciprocally stimulate those resident at the physical laboratory, there shall be established not more than twenty *temporary non-resident associateships of research.* They shall be recommended to the Trustees by the Council, after consultation with the physicist in

charge in whose department the candidate is eminent. The recommendation shall be accompanied by the scientific history of the candidate.

These associateships shall be granted to men of the recognized exceptional eminence already specified, connected with other institutions of learning, in consideration of their past and present continuous services to scientific research in physics.

It is hoped that in the interest of the advance of science, the person in question will be found deserving of additional encouragement on the part of the university or college to which he belongs, which encouragement may be an increase in salary, or relief from excessive teaching, or from routine duties, or any other reciprocal courtesy. It is to be specifically understood that the associateship is to be an *addition* to whatever material rank the recipient may have previously attained at his own institution.

The associateships shall be filled only when the person of the recognized ability has been found. They need not be filled continuously.

The temporary associateships shall consist of a grant of two thousand dollars per annum and be subject to renewal every five years, and after five renewals they shall be permanent and constitute a pension.

In return for the honor extended to the associate by the Carnegie Institution, the Council will ask for no further service than a connected annual account from the recipient, of the work on which during the term of his appointment he has been engaged. Each such account shall be given with the necessary scientific rigor and they shall together constitute his qualification for reëlection.

(14) *Resident associates.* In addition to the non-resident associateships, there shall be not more than fifteen resident associateships established at an average salary of $3,000 per annum. These associateships will probably fall to the lot of those showing most ability among the resident assistants hereinafter specified. The appointment shall be made for three years, after which it may be renewed.

The resident associates shall be nominated by the Council, after consultation with the physicist in charge in whose department the assistant has worked, and the Council shall state the scientific accomplishments of the candidate.

(15) *Research assistants.*—In order further to encourage research throughout the country, it is recommended that the Council be

empowered, after consultation with the physicists in charge, to nominate not more than twenty research assistantships open to young men of promise.

The recipients must have at least attained the equivalent of the highest degree at an institution recognized by the Council; they must have done a high character of original work, and be properly endorsed.

The research assistants shall be at liberty to pursue their investigations, whether theoretical or otherwise, under the supervision of the Council, either at the physical laboratory of the Carnegie Institution or elsewhere.

The salary of the research assistants shall be an average grant of $1,500 per annum, and the appointment shall be for two years, after which it may be renewed.

Endorsements of research assistants shall come from the physicists in charge, the temporary and permanent research associates, resident and non-resident.

(C) Grants.

The sum of 10,000 dollars is annually to be set apart to be expended at the discretion of the Council in the form of smaller grants, distributed to aid the advance of promising researches in physics.

The applicant for a grant must be at least of the grade of experience of a *research assistant;* he must be well endorsed, and his project clearly stated.

The Council shall not pledge itself to recommend an annual grant for an indefinite period, unless the application comes from a physical society of national standing, and it shall then use its discretion.

A scientific account of the work done with the aid of a grant shall be submitted to the Institution for publication, and the Council shall be empowered to call for such an account.

(D) Tabulated Summary.

An itemized statement of the costs of the laboratory, personnel, equipment, etc., is given in the following table:

Laboratory.	Physical Laboratory	$400,000
	Grounds	50,000
	Power house and shops, etc.	50,000
	Total	**$500,000**
	Original equipment	**$100,000**

This amount is to be collected gradually out of the annuity of $150,000 specified above. With the laboratory, etc., constructed, the annuity is to be expended as follows:

RESIDENT APPOINTMENTS.		
	Salaries of: Director	$8,000
	Physicists in charge, 5 @ $6000	30,000
	Executive officer	4,000
	Aids, foreman, electrician, laborers, etc	5,000
	Resident research associates, 10 @ $3,000	30,000
	Resident research assistants, 20 @ $1,500	30,000
	Instrument makers, etc	2,000
	Janitors and carpenters	2,000
	Clerks and computers	3,000
	Total annual running expenses and additions to equipment	30,000
	Unforeseen expenses	6,000
	Total	**$150,000**

To this is to be added the expense incurred outside of the laboratory, as follows:

NON-RESIDENT APPOINTMENTS.		
	Annuities for: Permanent non-resident associateships, with assistants and laboratory expenses, 4 at $10,000 each	$40,000
	Temporary non-resident associateships, 20 at $2,000 each	40,000
	Councillors, 5 at $2,000	10,000
	Grants to aid research	10,000
	Total	**$100,000**

IV. Provisional Plan.

To inaugurate the project which your Committee has just explained, it will be necessary to appoint a Council possessing the qualifications and entrusted with the duties above set forth. The fees of each member are to be $2,000 per annum.

With regard to the laboratory, your Committee think it prudent to begin with not more than one department. This it seems to them should be the department of light and radiation, since it is in the domain of optics that American research has been peculiarly fruitful and advanced, and a larger number of sufficiently able investigators is most likely to be available. The annual appropriation of $150,000 specified above, should be used to appoint the physicist in charge, the executive officer and his aids, and two research associates in residence; the remainder, which will considerably exceed $100,000, should be devoted to building purposes. The cost of the laboratory

for optical research is estimated at $125,000, and its equipment at $30,000. When the laboratory is sufficiently advanced and equipped, a third research associate and six research assistants in residence, together with instrument makers, clerks, computers, etc., should be appointed.

With regard to appointments not in residence, your Committee after prolonged and careful consideration, with testimony, recommend that in view of the opportunities of the laboratory, but one permanent research associateship and seven temporary research associateships be established at once. This implies an outlay of $34,000.

Finally the Committee are of the opinion that $10,000 should be made available for grants.

The total expenditure for the first year would therefore be $200,000.

*   *   *   *   *   *   *

Respectfully submitted.

R. S. WOODWARD, *Chairman*.
CARL BARUS.
A. A. MICHELSON.
Committee.

SEPTEMBER 26, 1902.

REPORT OF ADVISORY COMMITTEE ON GEOLOGY

To the Board of Managers of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Geology is of the opinion—

(*a*) That research in Geology is fairly well provided for by existing agencies.

(*b*) That the fundamental problems involving research in which Geology, Physics, and Chemistry must be more or less combined should be referred to the Advisory Committee on Geophysics.

(*c*) That certain special investigations and explorations in little known areas should be taken up by the Carnegie Institution.

In conformity with the above, the grants shown in the accompanying exhibit* are recommended for the fiscal year 1903.

Respectfully submitted by

T. C. CHAMBERLIN, *Chairman*,
CHAS. R. VAN HISE,
CHAS. D. WALCOTT,
Committee.

* Not here printed.

REPORT OF ADVISORY COMMITTEE ON GEOPHYSICS

CONTENTS.

I. INTRODUCTORY STATEMENT.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee to whom were referred the several questions relative to researches in geophysics, have had the same under very careful and prolonged deliberation, and submit herewith their conclusions. The Committee on Physics and Geology have jointly considered all matters relative to geophysical researches, while the Committee on Geology has considered questions essentially geological, and the deliberations of the Committee on Physics have been wholly independent. The joint deliberations of the committees on the actual problems presented have developed the fact that many of the geological and geophysical problems blend into each other so intimately as to offer no good cleavage line for practical separation, while the researches in pure physics are definitely separable from geophysics. Our final conclusion is that the researches in geophysics should be intimately associated with those in geology in administration, while those in physics should be administered independently.

The attention of the committee was largely occupied in considering the most advantageous method of providing for the experimentation which is indispensable to the progress of the profounder studies of earth problems.

Until recently the natural sciences and physical sciences have been handled as if almost independent of each other. The ground between has been largely neglected. The occupancy of this ground is certain to lead to important results. The order of results to be expected is illustrated by the great advances which have recently come from occupying the middle ground between astronomy and physics, and between physics and chemistry. For a long time astronomy and physics were pursued as independent sciences. The recent great discoveries of astro-physics have shown the advantages of their combination. Chemistry and physics for a long time were pursued as independent sciences. The rapid rise of physical chemistry has shown how wonderfully fruitful is the ground between the two.

No individual, university, or State has attempted to study in a comprehensive way the great territory between geology, physics and chemistry. Nor, so far as we know, does an individual, university, or State contemplate the attempt. This great, almost untouched territory, can only be occupied by geophysical laboratories properly equipped and manned.

Your committees have therefore given prolonged consideration to the following propositions:

(1) That a central laboratory of geophysics be established at Washington;

(2) That from this central laboratory the co-operation be sought of all independent laboratories engaged in geophysical studies, without reference to country;

(3) That, where necessary, branch laboratories be constructed in various parts of the world.

The thought has thus been to provide for geophysical work otherwise neglected, and to bring all the work done on this line by the Carnegie Institution under a unified and harmonious system.

(*a*) *The need for laboratories of geophysics*. Relative to the need to be supplied, we beg to reiterate that there are nowhere laboratories that are at all commensurate with the problems to be undertaken, nor is there any reasonable expectation that any established institution will undertake the great task of founding and maintaining such laboratories. This arises from the exceptional nature of investigations applicable to so great and so complex a body as the earth. The problems of geophysics and geochemistry involve the applications of pure physics and pure chemistry from the minutest parts of the earth to the mass of the earth as a whole, and even to

other celestial bodies. The earth presents the grandest concrete example, and the grandest congeries of concrete examples, of the properties of matter available to us. The phenomena presented by the earth are the historical products of chemical and physical forces. From the phenomena we infer the nature of the chemical and physical forces which produced them. However, these inferences require confirmation or refutation by special chemical and physical tests. Usually the phenomena are complex, and it is impracticable to decide between a number of hypotheses as to the manner in which the chemical and physical forces have accomplished the results. In such cases experimental work is absolutely essential in order to make correct inferences as to the nature and manner of work of the forces which have produced the phenomena observed. Moreover, the history of the earth, in large part well preserved in the rocks, is undoubtedly typical of the history of the other planets of the solar system, and hence affords a key to the problems of stellar systems in general.

Geophysics, using the term broadly to include geochemistry and related sciences, is founded on pure physics and chemistry ; its data are supplied chiefly by geology ; and the ramifications of its superstructure extend into astronomy and astrophysics.

The principles of physics have been demonstrated experimentally for only a very limited range of conditions. The experiments of physical laboratories usually relate to small masses only, under ordinary pressures and moderate temperatures, with brief time limits. The problems of the earth involve immense masses, extraordinary pressures, very high temperatures and enormous lapses of time. Before the results of ordinary experiments can be safely applied to the great earth problems, they must be tested for the most extreme ranges of mass, pressure, temperature and time that can be commanded. It is quite certain that nearly all the deductions from experiments under common surface conditions are subject to modification under the extraordinary conditions of the earth's interior, and the nature of these modifications must be determined before safe application of the deductions can be made. It is obvious that the determinations reached by such geophisical researches will be contributions of a high order of value to the science of physics itself.

(*b*) *The province of laboratories of geophysics.* There are three great fields which should be embraced within the researches of the laboratory. These blend into each other and embrace many minor fields and the researches in each will contribute to those of the oth-

ers, while the questions in each will stimulate and react helpfully upon one another. These are (1) *the great envelopes of the earth*, the atmosphere and the hydrosphere, which constitute the chief sources of external activities and condition the habitability of the globe; (2) *the body of the earth*, whose crust records its history, and whose interior is full of dark, intricate problems; and (3) *the motions and external relations of the earth*, which condition its form, its external temperature, its illumination, and probably its magnetism and other important phenomena. These lie in the common ground of several sciences, notably in the over-lapping border-fields of physics, chemistry, geology and astronomy, but they present special problems whose central interest is terrestrial.

The only hope of adequate solution of these profound problems lies in special experimentation. Neither the application of existing science, with its limitations, nor of pure theory, with its present narrow basis, covers, in any competent way, the ground of these problems. Their magnitude and complexity are such as to require a degree of extension of deductions quite beyond the limits warranted by present experimental data. It is therefore necessary that special experiments shall be devised to determine with the utmost precision the laws of variation, with varying conditions, under as great a range of mass, pressure, temperature and time as possible, and then to apply these determinations with the most critical circumspection to the phenomena which the earth itself presents, and to check them by cross-investigation based upon independent sources. The soul of the method should be to determine by the grandest and at the same time the most refined artificial experiments what is the meaning of the magnificent experiments expressed in the evolution of the earth. The experimentation should therefore be shaped with special reference to the phenomena of the earth, and be checked by all the cross-lines of evidence that can be drawn from geological and astronomical data.

(*c*) *Some of the problems to be investigated.* It is quite beyond the scope of this report to set forth adequately the particular questions to be investigated, but the mention of some of the more declared ones may serve to give definiteness to the undertaking recommended.

1. The envelopes of the earth doubtless in large measure had a common origin, and they are still most intimately related in action and function. In a narrow sense the atmosphere is the domain of meteorology, but its greater problems reach back into the domains

of geology, geophysics and astronomy. Among its sailent questions are those of its origin, its mass, its mass-limitations, its mass-distribution, the potential atmosphere absorbed in the ocean and in the body of the earth, its sources of depletion and enrichment, the constancy or fluctuation of its volume and of its constituents, its function as a thermal blanket, the possible changes in its diathermacy, and the relations of these to great climatic chances, together with many related problems that enter profoundly into the interpretations of the earth's past, and seem to have immense importance to the future of our race.

In these problems of the atmosphere, the hydrosphere participates. It also presents questions of its own as to its origin, mass and mass-distribution ; the constancy or variation of the volume of the ocean ; the possible function of the waters in promoting their own seggregation and the relation of this to the extension and withdrawal of the ocean relative to the land ; the part which the water-mass plays in the changes of form of the earth ; the origin, constancy, or variation of the ocean's salinity, the significance of this in past and future history, and many other questions.

The solution of nearly all of these problems rests ultimately upon grounds that need the searching tests of the laboratory. For example, a rigorous determination of the diathermacy of the air, and of its dependence on the several atmospheric constituents, and on their relations to each other, their ionization, their nucleation and their other states, is just now most urgently needed. So is also a critical inquiry into the part the ocean is competent to play in absorbing and giving forth atmospheric constituents under different conditions of temperature, pressure, tension, salinity and biological content.

2. The crust of the lithosphere has thus far been the chief field of geology in the narrower sense, since it contains the rock record of the earth's past, and geological studies have peen directed chiefly to reading and mapping this record. But the record needs to be interpreted on broader and deeper lines, based on a profounder knowledge of physical laws. To this end the data of geology need to be correlated and unified under these laws on an experimental basis. Apart from the record in the layers of sediment, there is a recondite but less legible record in the dynamic features of the earth, and to read these successfully will require the utmost resources of research, involving the fullest available aid of geodetic, physico-chemical and mathematico-physical investigation.

Some of the salient problems of the outer lithosphere are the origin and maintenance of the continental platforms—with their superposed mountains and plateaus—and the abyssmal basins, involving questions of rigidity, isostasy, etc.; the agencies and conditions that make possible the prolonged periods of crustal quiescence shown in baseleveling, and the antithetical epochs of crustal disturbance; the sources of crustal displacement and distortion, shown intensely in the faulting and in the crumpling of mountains and plateaus, and shown massively in the continents and oceanic basins; the mashing, shearing, and foliation of the rocks leading on to the general problems of metamorphism, and a whole group of intricate questions of a chemical and chemico-physical nature, including the flow of rocks, the destruction and genesis of minerals, the functions of included water and gases, the internal transfer of material, the origin of ore deposits, the evolution and absorption of heat, and other phenomena that involve the effects of temperature, pressure, tension, and resultant distortion upon chemical changes and mineralogical aggregations.

These questions of the earth's outer part are inseparably bound up with those of the interior, and here the problems involve the most extreme and the least known conditions, and make their strongest demand for experimental light. The themes here are the kinds and distribution of the lithic and metallic materials in the deep interior; the states of the matter; the distribution of mass and of density, and the consequent distribution of pressure; the origin and distribution of heat; the conductivities of the interior material under the pressure and heat to which it is subjected; the heat possibilities arising from supposed original gaseous condensation, or alternately from initial impact of aggregation; the heat of subsequent attractional condensation; the secular redistribution of heat within the earth, and its loss from the surface; the possible relations of redistribution of internal heat to vulcanism and to deformation, and similar profound problems.

A series of specific laboratory questions arise from these, e. g., the effect of pressure on the melting point of rocks carried to as high temperatures and pressures, and through as wide ranges of material as possible, to develop the laws of constancy or of variation; the effect of temperature and pressure on thermal conductivity as indicated above, and on elasticity, especially as involved in the transmission of seismic tremors.

One of the most hopeful resources for disclosing the constitution and conditions of the interior is found in the transmission of earthquake vibrations directly through the body of the earth, such as are now being recorded in different parts of the globe. These disclose extraordinary elasticity in the center of the earth, but before this fact can be safely interpreted the effects of pressure and heat on the elasticity of different classes of earth material in its different states need to be determined with the greatest practicable precision for the greatest practicable range of pressures, temperatures, materials and states of materials.

As a factor in the great problem of interior heat, the compressibility of rock material, the amounts of heat developed in compression and recrystallization need careful determination, and, if possible, also the question whether compressibility has definite limitations or not.

Intimately related to the problems outlined above, and to a great extent doubtless entangled with them, are the problems of physical geodesy and terrestrial magnetism. Important light on the distribution of mass in the crust and nucleus of the earth must be derived from an extended gravimetric survey of the earth's surface. The various national geodetic surveys may be depended on to furnish an increasing amount of data in gravimetric measures, but it ought to be one of the functions of a geophysical laboratory to assist actively in such work, which is largely incidental to the operations of geodesy.

Similarly the problem of terrestrial magnetism belongs rather with geophysics than geodesy. It appears to be indeed, in some of its aspects, a cosmological question, and should be studied as an important problem in itself rather than as one incident to geodetic and topographic work.

3. The relation of the earth to neighboring bodies, its motions and the modification of its form imposed by these, also constitute a record of the earth's history, and a forecast of its future, since the organization of a planet or a planetary system is as much the record of past events as the organization of a rock or a system of sediments, but it is extremely difficult to read. It can only be deciphered positively when the sign manual of planetary dynamics is made as legible as that of the water sediments is now by critical research, chiefly of a mathematico-physical type, aided by laboratory verification. For example, the relations of the earth and moon and their rotations undoubtedly record a history of tidal reactions and of

mutual separation and the reduction of rotations, but just how much this may signify in the history of the past and of the future requires a new and much broader inquiry than it has yet received, checked by independent lines of evidence. The question whether the earth is now bodily deformed by tidal stress requires experimental demonstration or overthrow, as an essential factor in this problem. This would test the effective rigidity of the earth, and give aid to the solution of other problems. The motion of the moon and the precession of the equinoxes perhaps give lines of approach to the distribution of mass in the earth, and hence to its internal density, etc. The origin of the size, form, constitution, motions and relations of the earth are locked up with the origin of the solar system, and, in general, the deeper terrestrial questions lead out in the end into the realm of cosmology, where the studies of the geologist, astronomer, physicist and chemist blend. Geophysical study must here borrow much from astronomy, but it should make an equivalent return, for the phenomena of the earth are most important factors in cosmology. Any great laboratory of geophysics should do its part in this vast field.

(*d*) *Some of the more specific problems now pressing for solution.*

1. Experiments to demonstrate the diathermacy of the atmosphere and its dependence upon its several constituents, their relations to each other, their ionization, their nucleation, and their other states.

2. Determinations of the gases held in magmas, rocks and meteorites, and the states in which they are held, together with inquiry into the powers of selection and absorption of gases by rocks under ordinary and unusual conditions.

3. Determinations of the functions of the ocean as a reservoir of atmospheric material, involving a study of the relations of its saline constituents to the absorption and release of atmospheric constituents, the relations of temperature and pressure to such absorption and release, as also the functions of vegetable and animal life in the process.

4. Experiments to determine the physical chemistry of natural solutions and precipitates; one important purpose being to furnish a basis for a more comprehensive science of ore deposits.

5. The artificial alteration and recrystallization of minerals under different chemical and physical conditions, in imitation and elucidation of the natural alteration of minerals.

6. The determination of the heat of formation of all natural compounds.

7. Experiments in the deformation of rocks under conditions of great stress, not only in one direction, but with unequal stresses in different directions, and under wide ranges of temperature, moisture and other conditions.

8. Determinations of the relations of pressure to the melting point while under differential stress and other variable conditions, including variable amounts of water, vapors and gases.

9. Determinations of the conductivity of rocks and the laws of variation of such conductivity under varying conditions of heat and pressure.

10. Determinations of the elasticity of rocks and the laws of variation of elasticity under varying conditions of heat, pressure, change of state and change of substance, involving also experiments on the compressibility of rocks.

11. Experiments and mathematical investigations to determine the nature and quantitative value of the possible sources of internal heat under multiple hypotheses as to the original states of the earth.

12. Determinations of the original distributions of heat under such hypotheses, of the secular loss, of the secular generation of heat by gravitative condensation, of the redistribution of internal heat and its possible relations to deformation and vulcanism.

13. Tidal deformation by observational determinations in laboratory and field.

14. Mathematico-physical reinvestigations of the moon-earth tidal relationship, and its bearings on the past and prospective history of the earth, wrought out under multiple hypotheses covering the full limits of the probabilities of the case.

15. Tests of the distribution of the internal densities, or mass-distribution of the earth by astronomic data.

16. Gravimetric measures at specially selected significant points embracing (1) such points as will best determine the distribution of gravity upon the ocean areas as distinguished from the continental, and on the border ground between these, and (2) at such points as show notable variations of increase of internal temperature in depth (independent of obvious recent volcanic action) to determine whether the observable variations are dependent on variations of density, and so possibly are dependent on compression.

II. Laboratory of Geophysics in Washington.

In view of the special nature of the geophysical and geochemical experimentation which the elucidation of the profounder problems of the earth requires, and of the absence of many of the requisite appliances in the laboratories now established, we recommend that a central laboratary of geophysics be established by the Carnegie Institution at Washington, and that its scope be broad enough to embrace geochemistry and any other science essentially involved in the problems of the earth. We designate this a central laboratory because we recognize the need of special branch laboratories in various parts of the world for the determination of certain questions which require special localization. We further recognize that alliance and co-operation should be sought with all independent laboratories engaged in any branch of geophysical studies without reference to country. So far as practicable such laboratories should be utilized rather than new branch laboratories be constructed. The central and branch laboratories should be constructed for the special investigations for which they are designed, and should be manned with reference to the problems to be investigated.

If this project be carried out the geophysical work of the world may be harmonized and unified. It is believed that as a certain result geology will soon be placed on a broader and deeper basis, and that an epoch in its history will have been inaugurated of even greater import than any of the past.

For the geophysical work outside of the central laboratory, as already remarked, existing laboratories and the services of men engaged in them should be utilized as far as possible. For instance, in the seismological work—including all earth tremors—a scheme of co-operation should be planned with Milne, Darwin, and many others. In securing the co-operation of independent laboratories now existing, it will doubtless be necessary to subsidize to some extent such as are doing very meritorious work. Often a man engaged in a piece of research is the best qualified to do that particular work. Such a man should be encouraged to do the service which he can most advantageously perform ; thus would be utilized the best talent wherever located or however associated, Indeed, it is felt that a vital part of this proposal for a system of Carnegie laboratories of geophysics is the development of a staff and a directory at Washington, which may serve as a center of correlation, through

whose good offices the co-ordination and co-operation of all laboratories doing chemical or physical work bearing upon geology throughout the world may be secured.

(*a*) *Construction of the laboratory*. It seems to us that it would be advisable that the laboratory building should be planned so as to consist of a series of units, not necessarily identical, that can be indefinitely attached to each other with good architectural effects, so that a working portion of the laboratories can be brought into use as early as possible with the minimum expense, while extensions can be made from time to time as funds permit. The units should be so planned that they will be essentially workable in themselves, while at the same time they should be as modest in expense as practicable, so as to give as great adaptability to financial resources as may be, and at the same time permit advantage to be taken of all new developments in buildings and appliances.

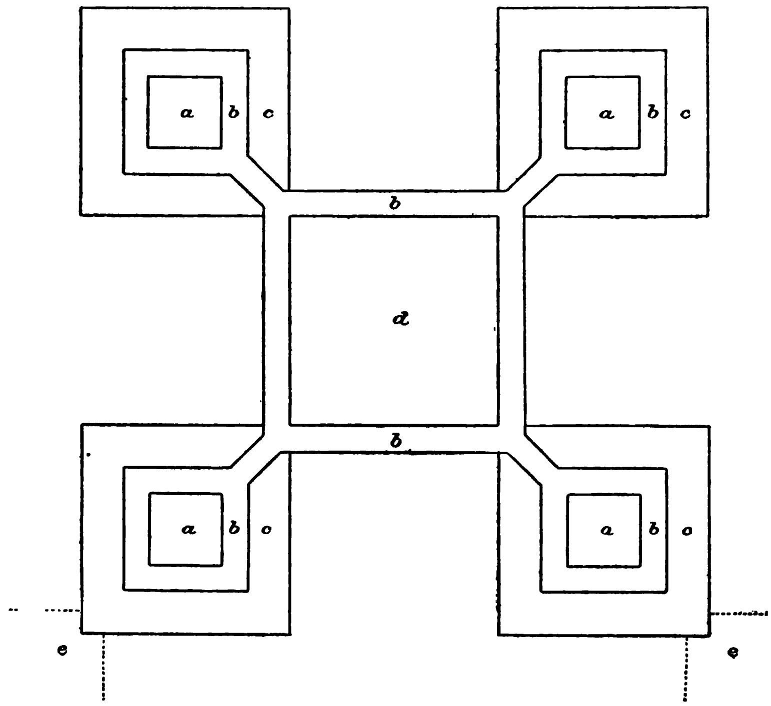
They should provide for uniform-temperature laboratories, rock-bottomed laboratories, ordinary laboratories, workrooms, libraries, offices, etc.

The units should be so intimately united as to be in effect portions of one building, and yet so readily isolable as to exclude undesirable but inevitable effects of work in one class from interference with work in neighboring units.

The exterior walls should be protected against the sun and rain by wide cornices and balconies.

Uniform temperature for experiments, and reasonably uniform temperature for comfortable and effective working should be secured by special means of air control and air supply; perhaps either by carrying the air (a) through underground conduits, or (b) through special chambers cooled or heated, as required, artificially. The necessity for uniform-temperature laboratories for certain classes of experimentation is recognized, but perhaps not equally the desirability of air controlled in purity, temperature, and moisture for the purpose of securing the highest efficiency of intellectual procedure. The most serious objection to the erection of the laboratories in Washington is climatic. This can probably be overcome in some large measure by artificial appliances, and the importance of securing this as a factor in realizing the highest intellectual results should, in our judgment, receive the earnest consideration of the Trustees.

We submit herewith sketches of two out of the many forms of combination that may be assumed by the proposed unit system of building.



EXPLANATION.

SKETCH I.

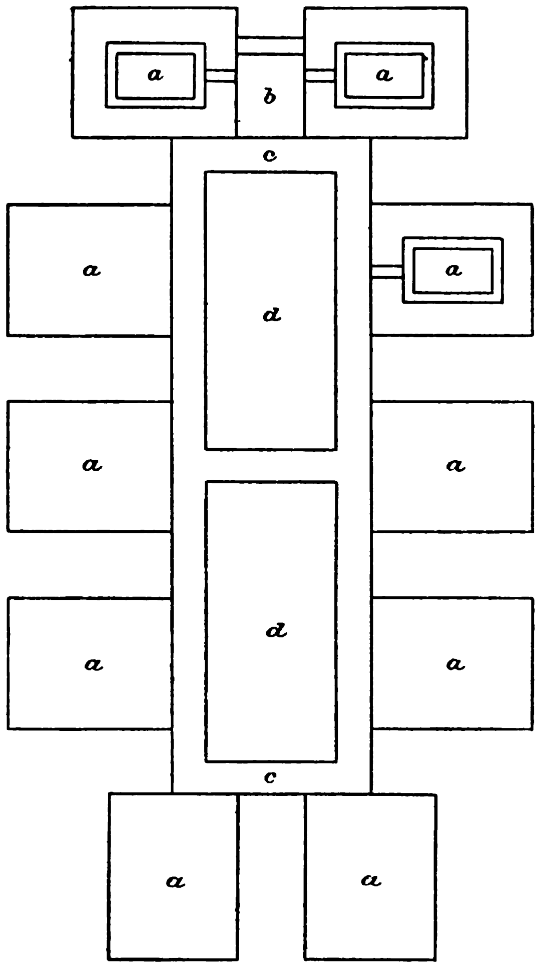
aaaa Uniform temperature laboratories 28′ x 28′, protected by corridors (8 ft., including walls) and work rooms, common laboratories, offices, libraries, etc., *c c c c* outside. To be lighted (a) from above and (b) artificially.

bbbb Corridors for communication, but also to serve as insulators for uniform-temperature laboratories (8 ft., including walls).

cccc Space lighted from without for work rooms, ordinary laboratories, offices, libraries, etc., (18 ft. wide, including outer wall). To be divided by removable partitions (tile or Michaelite) adjusted to needs.

d Central unit that can be treated as the bastion unit, or can be developed as a large (60 ft. x 80 ft.) measurably uniform-temperature laboratory, either lighted from (a) above, (b) laterally through two sets of windows, or (c) artificially.

ee Extension by the addition of units similar to units *a–c*.



EXPLANATION.

SKETCH II.

aaaa Units 80′ x 100′, to be developed as in Sketch I, or variations from that.

b Vestibule hall, stairway, public office, etc., 60′ x 40′.

cc Portico connecting the laboratories on ground and second floors.

dd Courts enclosed by portico. To be an uninterrupted colonnade on inner side facing court, and to be a double colonnade between the buildings, to permit air circulation.

(*b*) *Administration*. The administration of such investigations as are here contemplated should be as simple as possible, consistent with efficiency in the prosecution of research. Those engaged in research work should be as free as may be from the requirements of fiscal business. The success of the enterprise will largely depend upon the men who undertake research, and it may not be easy at first to find men in all respects specially qualified for the work. To some extent they must be developed by the institution itself.

The special methods of administration likewise must be to a large extent developed by the novel scope of the undertaking. Since much must, therefore, be left to future developments, it seems best that the original form of administration should be such as will bring together a large measure of experience and proven competency, and at the same time be such as will permit a change of form with the greatest ease. It is obvious that it will be less difficult to pass from administration by a body to administration by a head than the opposite. It is clear that no one at the outset can have the learning and the breadth necessary to decide on the respective merits of all the problems that will invite investigation, and such a genius may soon arise. For these reasons it would seem best, in the early history of the enterprise at any rate, to vest the control of affairs in a directory or committee made up of scientists who have shown at once administrative abilities, fertility in the development of problems, ingenuity in devising modes of solution, and success in the achievement of results.

The directory should meet at least quarterly, and as much oftener as the nature of its duties may require.

The immediate administration of the laboratory should rest in a head or acting head The head should be either a geologist, with a broad and accurate knowledge of many branches of geology, and also well grounded in the principles of physics and chemistry, or else a general physicist, who has deeply studied the broad problems of the earth, and, therefore, has a working knowledge of the principles of geology. The head should be elected by the Trustees, or executive committee upon recommendation of the directory. The election of the staff other than the head may be committed to the directory, or the directory may recommend the staff to be appointed as seems best.

It should be the function of the directory, subject to such rules and limitations as may be determined upon, to consider all proposals for investigation, to devise or mature all general plans for research,

to pass upon all plans for buildings and other appointments needed for research, to recommend the apportionment of funds to research, to buildings, to equipment and to other purposes, to supervise the publication of results, and in general to represent the executive committee in the expert administration of the investigations and of the provisions therefor. They should make such reports to the executive committee as the committee may direct.

It is important that the relations between the executive committee and the directory should be as simple as practicable, and that responsibility should be definitely located, and the proper powers for meeting responsibility should be granted. It is, therefore, suggested that the directory be made responsible for the suballotment of such funds as may be appropriated to the several researches. These will inevitably sometimes exceed and sometimes fall short of the estimates made, and in order to secure the best results, the plans adopted will often require modification in the course of their execution. It is obvious that provision should be made for as large a use of scientific discretion in the carrying out of the project as is practicable; especially is this true at the outset, when plans should remain in large measure plastic. In the determination of investigations to be undertaken, and especially in the construction and the manning of the laboratories a slow and conservative course should be followed. Buildings should only be constructed after an exhaustive study of existing laboratories, and a careful consideration of the most promising lines of research. At the outset only those men should be given places in the laboratory who by their services or by their qualities have shown themselves eminently capable of fruitfully carrying on the investigations desired. The relations between the investigations to be taken up in the laboratory and the investigations to be taken up in other laboratories or in the field must be carefully considered individually and collectively, and be so arranged as to be mutually as helpful as possible.

It is inadvisable to map out beforehand a hard and fast apportionment of funds to buildings, equipment and researches. Such an apportionment should be reached step by step as the enterprise develops and the plans approve themselves in actual results. It would seem therefore best that general allotments based upon estimates by the directory should be made for the researches and that the sub-apportionment of these should be left to the directory. For example, if a round sum were set apart to cover buildings, equip-

ment, and researches, the directory could adjust the actual expenditures to buildings, equipment and research in such a way as to secure the best adaptations and most fruitful results. This suggestion would at once simplify the work of the executive committee, while it will confer responsibility and corresponding facilities upon the directory. Under this plan it would be possible for the trustees to fix upon a definite yearly apportionment to the researches indicated.

These suggestions are in the main equally applicable to the administration of researches in geology, physics, and other sciences in respect to which a common plan will doubtless be adopted. They also lead to the consideration of the administrative limits of departments and the working relations between the departments, as these must be determined in selecting formal modes of control.

Relative to the definition of departments, their working relations to eaeh other, and the specific forms of administration of the investigations in geology, geophysics, and physics, three plans have been under consideration by the joint committee:

(1) The first plan proposes that there be directories corresponding to the existing advisory committees.

(2) The second plan contemplates the merging of the administration of the investigations in geophysics and geology in a common enterprise, to be administered as a unit by a single directory.

(3) The third plan approved by the committee lies between these two, and proposes a directory for geology and geophysics in two sections of largely identical membership, the section of the directory in charge of investigations in geology to consist of three members, who shall be experienced geologists of wide familiarity with geological problems; the section of the directory in charge of the geophysical laboratory to consist of the above, the head of the proposed laboratory of geophysics as an ex-officio member; also a member of the board of trustees of the Carnegie Institution as an ex-officio member, together with an expert physicist, and an expert chemist, these two to be presumably, though not necessarily, members of the directory in pure physics and pure chemistry, for the purposes of correlation, as above indicated.

This plan contemplates separate allotments for the geophysical and the geological phases of the work to be administered by the two sections of the directory, respectively, but in the closest co-operation, as implied by the large factor of common membership

(*c*) *Expense involved.* In advance of an exhaustive investigation it is impossible to give exact estimates for particular parts of the proposed work; but we are able to give an approximate estimate of the minimum amount which will adequately provide for the entire plan. Our estimates are based on the large idea that a great unoccupied field is to be provided for; but at the same time, on the certainty that with the largest sum which we could reasonably hope would be appropriated, it will be necessary to administer the fund with great care and economy, limiting the investigation to pressing questions of fruitful promise.

At the outset a considerable sum will be necessary for construction, but only a comparatively small sum can be used very wisely for investigation. As the central laboratory approaches completion, the building expenses will be greatly decreased, but the compensation for investigators and the maintenance of the laboratories will necessarily be greatly increased. By proper administration the projects here proposed may be carried out by a uniform annual expenditure.

On the supposition that a system of building by units, as elsewhere stated, is adopted, it is estimated that each unit would cost from $100,000 to $125,000, and its initial equipment from $12,000 to $15,000. It is thought that three units should be erected during the first five years, and two additional during the next five years.

It is estimated that salaries and maintenance, including current cost of experimentation, books, printing, heating, lighting, etc., should rise from $25,000 or $35,000 the first year to the maximum available, which ought to reach $150,000 within the first ten years, to be commensurate with the needs of the field.

An appropriation of $150,000 per year would develop and carry forward the work in a very advantageous manner.

The development of the plan could be somewhat slower than contemplated in these estimates by adding units to the building at greater intervals, which would carry with it a slower increment in the staff and in the cost of maintenance, with, of course, a slower realization of results.

The expense of special laboratories can only be determined as the occasion for them shall be developed. So far as existing laboratories and stations can be utilized, modest auxiliary allotments will usually suffice.

Estimate of cost of laboratories and running expenses per annum, $150,000.

We submit herewith two appendices, viz :—

Appendix 1. A carefully detailed estimate for the proposed central laboratory, prepared by Dr. George F. Becker, which does not, however, cover all lines of research contemplated in our report.

Appendix 2. Communications from distinguished scientists of different fields relative to the value and functions of a geophysical laboratory.

In conclusion, we beg to express to the trustees of the Carnegie Institution our profound appreciation of the greatness of the enterprise they have in charge. We believe it not too much to think that it will mark a new era in the intellectual development of the race, an era in which scientific research will hold the foremost place in the agencies of progress and, we may hope, will be given the highest place in the esteem of mankind.

We venture to believe, also, that if the facilities herein recommended are granted, it will assure to America the foremost place in the special fields for which, through your kindness, we have been permitted to plan.

Respectfully submitted,

R. S. WOODWARD, *Chairman*,
CARL BARUS,
T. C. CHAMBERLIN,
A. A. MICHELSON,
C. R. VAN HISE,
CHAS. D. WALCOTT.
Committee.

SEPTEMBER 23, 1902.

APPENDIX 1 TO REPORT OF ADVISORY COMMITTEE ON GEOPHYSICS.

PROJECT FOR A GEOPHYSICAL LABORATORY.

By George F. Becker.

Contents.

PART I.—OUTLINE OF GEOPHYSICAL RESEARCH.

PART II.—PLANT REQUIRED.

PART I. OUTLINE OF GEOPHYSICAL RESEARCH.

1. Scope of the undertaking.—The purpose of a geophysical laboratory is to supply a firm scientific foundation for the study of the past history and the present condition of the earth. The researches in such a laboratory can consist only in part in the application of known theories to terrestrial problems, because in many most important cases the theoretical basis is imperfect or altogether wanting.

Geophysics covers a distinct group of problems which are so closely related to one another in their nature and in the methods applicable to their solution as to stand somewhat apart from general physics. These problems demand for their successful solution simultaneous study and the carefully organized co-operation of a number of investigators over a series of years.

It is because these conditions have hitherto been lacking that so much remains to be done in this direction.

2. *Some of the problems of geophysics.*—To illustrate this view of the matter brief comments may be submitted on three of the many great questions of geophysics, namely, the distribution of terrestrial density, upheaval and subsidence, and vulcanism. All of them are subjects of extreme difficulty, but not, in my opinion, beyond the reach of well directed efforts.

The distribution of densities evidently depends upon the materials of which the globe is composed, the temperatures which prevail at different depths, and the elastic properties of matter. The possible laws of terrestrial density cannot be intelligently discussed beyond the point where Laplace left the subject until the law of elasticity for finite stresses has been elucidated.

Some suggestive work on elasticity has recently been done in the Division of Chemical and Physical Research of the U. S. Geological Survey along the lines of a former investigation of my own, and is stiil in progress there. While a large amount of confirmatory experimentation is still requisite, enough has been accomplished to indicate the character of the result.

The law of elasticity for constant temperatures having been discovered, it will then be necessary to determine the functional relations of elasticity and temperature. And since the range of temperature is necessarily large, considerable researches at high temperatures must accompany the study of the finite stress-strain function.

Supposing the relations known, a comparison of the earth, the moon and Mars can be employed to test the more probable theories of composition ; for example, that which considers the planets, like the meteorites, to be composed of nickel-steel and stony matter comparable with the average rock.

The theory of the vibrations of a sphere, when adequately developed, can be used still further to confirm or to upset working hypotheses as to the chief terrestrial constituents by taking advantage of the rate of transmission of earthquake shocks along various chords of the terrestrial sphere.

Evidently therefore the problem of the distribution of densities requires the combined efforts of at least three investigators—one devoted to elasticity, a second to high temperatures, and a third to the mathematical development.

The most hopeful line of attack on the problem of upheaval and subsidence appears to me to be as follows : A sphere is conceivable in which the distribution of thermal diffusivities and conductivi-

ties is such that the condition called by Fourier "the steady flow of heat," would subsist. In such a globe cooling would be accompanied by no superficial deformation. It is therefore essential to the solution of the problem that the character of such an ideal, undeformed sphere should be worked out from the mathematical side. If it can be shown that the constitution of the earth does not conincide with that of this hypothetical globe, a reason for deformation will have been discovered. As soon as an approximate solution has been attained for the constituent materials of the globe and their properties, then comparison with the hypothetical sphere of no deformation will show the general character of the deformation which should be expected in the earth.

It appears possible that some of the greater phenomena which must be discussed under the head of upheaval and subsidence are not connected with the cooling of the globe, but with the retardation of its rotation and the consequent changes in its form in maintaining equilibrium. It is a very remarkable fact, not recorded in literature, so far as I know, that most of the great continental outlines lie approximately on great circles which are nearly tangent to the Antarctic Continent and the Arctic Ocean. It seems possible that these great coast lines answer to the directions of rupture of a spheroid retarded by tidal action. A proper discussion of the matter requires a knowledge of the constituents of the earth and of the laws of rupture, which are as yet in a most unsatisfactory condition.

Orogenic dynamics is a mere branch of upheaval and subsidence involving the theory of elasticity, plasticity, and rupture for finite strains. It must explain the origin of joints, systems of veins, slaty or schistose cleavage and the simpler flexures.

It is evident, then, that the problem of upheaval and subsidence is linked by the closest ties to the problem of the earth's constitution and should be conducted by the same corps of physicists.

Our knowledge of the operative causes in vulcanism is of the vaguest description. It seems almost certain, however, that these impressive thermal phenomena cannot be wholly independent of the vast amount of energy dissipated in the orogenic upheavals which so often accompany eruptivity. The thermodynamic side of this problem therefore demands earnest study.

Vulcanism also raises other and very difficult questions. That magmas are solutions is known, but scarcely anything else is known about them. Thus, vulcanism implies researches on the

nature of igneous solutions, their chemical affinities, their ionization, latent heat, eutectic properties, etc. A special branch of this subject, but one of the most important, is the study of aquo-igneous fusion and the solutions resulting from it. The study of eutectic mixtures, including those into which the hydroxyl enters as a component, should lead to a classification of massive rocks on a new basis. Vulcanism also demands a study of the viscosity and diffusivity of solutions, especially of magmas, and of the relations which must subsist between them. The results obtained should be determinative with reference to the theory of the so-called "differentiation" of rock magmas. Diffusivity is inversely proportional to some function of viscosity, and there is some probability that this function is the square. Preparations are now in progress to test these relations in my laboratory by novel methods.

The cause of the extrusion or the intrusion of magmas is unknown. I am of the opinion at present that its origin is the elastic pressure of solid masses upon materials deprived of their rigidity by fusion. This may at least serve as a starting point for investigation. It appears then that vulcanism is probably allied to orogeny in an intimate manner.

Its study will require at least one additional physicist devoted to researches on the physical properties of solutions and of a physical chemist to undertake the more purely chemical properties of magmas.

3. Necessity for Organized Research in Geophysics.—This brief outline of the possibilities of systematic investigation in three of the most important problems of geophysics appears to me to justify the opinion that a special corps of investigators, with special laboratory facilities and under a single directorship, should be devoted to the study of geophysics. In my opinion, it would be beyond the power of any one man to co-ordinate the various branches of the work of a geophysical laboratory, while paying due regard to the needs of geological science, and at the same time to superintend any more extensive scheme of physical research. To be successful the work must be organized with reference to its special character, and even then the task will severely tax the best corps of men which can be enlisted. On the other hand, it is difficult to imagine contributions to general physics of more fundamental importance than those which would ensue from the successful prosecution of these geophysical researches.

4. Personnel Required.—Assuming, then, that a special laboratory, staff and director are requisite to the successful prosecution of geophysical researches, I offer the following estimates of the personnel and plant appropriate to such an institution, basing the plans largely upon the experience of the Physikalische Reichsanstalt of Charlottenburg, with modifications adapting them to American conditions.

The plan is in general terms as follows: In addition to a director, one mathematician, four experimental physical investigators, one chemical investigator and one analytical chemist are recommended. The necessity for a first-class mathematician is too evident to need comment. For years to come one physicist should confine himself to the study of elasticity, plasticity and rupture. A second physicist is required to take charge of high temperature work, beginning with an extension of thermometry to the melting point of platinum and the quantitative determination of the fundamental physical relations at those temperatures.

A third physicist should devote himself to the study of viscosity and diffusion, beginning with solutions at ordinary temperatures. Osmosis, with inorganic septa, and capillarity, two topics of very great importance to geophysics, should be entrusted to a fourth investigator. A complete geophysical laboratory must include at least one chemist to study the chemical relations of eutetic mixtures and investigate affinities at high temperatures. An analytical chemist is required for the numerous chemical analyses which will be called for in all branches of the work.

This corps of investigators must be provided with assistants to relieve them of the simpler details. Mechanical assistants and a small office force will also be required.

In estimating for the salaries of the principal members of the staff, I have taken as a basis the best salaries paid to college professors. Unless such salaries are paid there would be danger that the men might be tempted to abandon geophysics for college positions, much to the detriment of the proposed research. Investigators in geophysics must learn to take a somewhat novel attitude towards the science of the earth. They must attain a sufficient grasp of geology and its phenomena to perceive the demand for physical research, the application of its results and their relative importance. A man who has attained this unusual standpoint cannot readily be replaced either by a physicist unacquainted with geology or by a geologist insufficiently trained in physics.

I do not think that a man competent to investigate great problems should be asked to serve for less than $3,000 a year. Men who have earned large reputations must receive higher salaries, rising, say, to $6,000. The director might be worth $7,000. It is my understanding that some system of retiring pay is to be provided in the Carnegie Institution. I would suggest that if a fixed age of retirement is adopted, provision be made for exceptions in cases of unusual vigor, and, further, that retired officers should be expected to contribute to the publications of the Institution such material as they may find practicable without undue exertion.

5. *General features of plant.*—A geophysical laboratory to be satisfactory must be built upon a rock foundation in a locality as far removed as practicable from all mechanical and electrical disturbances (1,000 feet at least), and must be secure against the encroachment of disturbing conditions for the future. The success of secular experiments, the stability of instruments and the accuracy of electrical methods of measurement demand such conditions.

A satisfactory laboratory cannot be built in a brief period, and I have therefore given estimates for the distribution of the expenditures for plant, salaries and maintenance over a term of four years, supposing the fourth year to represent the permanent annual expenditure. These estimates would, of course, require careful revision by an architect and others, but may serve a preliminary purpose.

In closing what I can at present contribute to the subject, I have outlined some features of such an administration as experience seems to show should be adopted for the geophysical laboratory.

6. *Scientific staff.*

YEAR.	PERMANENT STAFF.		TEMPORARY STAFF.	
First......	Director.............	$6,000–8,000	Computor ...,..	$1,000–1,500
	Mathematician	3,000–6,000		
	Physicist.............	5,000–6,000		
	Chemist..............	3,000–6,000		
Second...	Director.......	6,000–8,000	Computor.......	1,000–1,500
	Mathematician.......	3,000–6,000	Ass't Phys......	1,000–1,800
	Physicist.............	3,000–6,000	Ass't Chem.....	1,000–1,800
	Chemist..............	3,000–6,000		

10. Summary of estimates.

	Salaries.	Plant.	Maintenance.	Totals (assuming mean salaries).
First year..................	$18,700—31,700	100,000	8,500	$133,700
Second year..................	20,700—35,300	100,000	12,500	140,500
Third year..................	34,580—60,680	96,000	19,000	162,630
Fourth and following years....	39,660—70,080	....	25,000	79,870

These estimates assume that land, power, heat, administrative expenses and maintenance of buildings are provided for in the general organization of the Institution.

11. Organization.—I take the liberty of recording here some notes on such an organization as appears to me suitable for a geophysical laboratory, established upon the Carnegie Foundation. Such a laboratory would naturally be under the control of the Executive Committee, but I would suggest that its immediate governing board consist of an independent visiting committee, comprising three physicists and three geologists not connected with the Institution, this board to be presided over by the director of the laboratory, who shall vote only in case of a tie.

The members of the visiting committee might be appointed for four-year terms, retiring in rotation. They might meet annually at a stated season, to consider the report of the director, and also, in urgent cases, upon call with due notice. Business requiring the attention of the committee between annual meetings, if not very urgent or complex, might be settled by correspondence. The members of the committee might receive their expenses and a per diem compensation for attendance at the meetings.

The director might lay before the committee at its annual meetings:

(1) A report of the scientific work of the year.

(2) A plan for scientific work for the ensuing year.

(3) Plans for the expenditure of funds for the ensuing year ; such plans being subject to the approval of the committee, excepting that one-fifth of the sum appropriated for research should constitute a contingent fund to be expended at the discretion of the director and duly accounted for by him.

(4) Nominations for vacancies in the permanent staff.

(5) Recommendations for the reception of volunteer assistants or visitors, whose co-operation may be desirable.

The decision of the visiting committee in matters pertaining to the salary of permanent members of the scientific staff should be final.

The director might further submit an annual report to the Executive Committee of all expenditures proposed for the approaching year, after this has received the approval of the visiting committee, and a report of the funds expended during the year upon the plan previously approved both by the visiting committee and the executive committee.

All appointments, excepting in the permanent scientific staff, might be considered temporary and terminable at the discretion of the director.

All expenditures might be subject to the director's order, he being responrible to the executive committee of the institute for all outlays.

All powers not 'directly exercised by the executive committee or the visiting committee would be vested in the director.

PART II.—PLANT REQUIRED.

12. Situation desired.—In the foregoing an effort has been made to show that these fundamental researches in geophysics are of a character to demand the combined efforts of several investigators, and a special laboratory suitably located, planned, and equipped for this work.

Some of the requirements of such a laboratory may be anticipated at once from the character of the problems to be considered, others are suggested by the German Reichsanstalt, where for several years research work of a high order has been carried on in pure physics; the final details can only be prepared with the help of an architect and after visiting the modern research laboratories.

The essential conditions for the prosecution of the work here contemplated are:

(1) The greatest possible freedom from mechanical and electrical disturbances.

(2) Effective provision against changes of temperature.

The first necessity is, therefore, the choice of a suitable location. The site selected should be high, to insure good light and dryness. and should offer a rock foundation for the laboratory building. It

should be remote from powerful electrical or mechanical plants, and should be surrounded by sufficient land under its control to secure the institution against the encroachment of such sources of disturbance for the future.

It is hardly possible that the present rapid extension of electrical tramways to place such a laboratary permanently beyond the reach of all electrical influence without removing to a point so inaccessible as seriously to inconvenience the daily life of those engaged in the work. Fortunately, however, the number of problems requiring conditions of extreme freedom from electrical disturbance is rather limited, and these have been left by common consent to small laboratories far removed from the centers of population, and equipped solely for this work. The extreme requirement in this direction may, therefore, be regarded as superfluous in the present plan.

The laboratory may be safely located within one thousand feet of a well insulated, underground trolly, or a *double* overhead system; ordinary overhead feeds, with return through surface conductors or through the rails, will cause a considerable magnetic disturbance at a distance of a mile with the passage of every car. Cars equipped with accumulators, electrical carriages, or systems of electrical lighting using direct current, exert little influence at a distance.

Nearly all the finer physical measurements are seriously disturbed by the jar of passing traffic, and by temperature changes. It is, therefore, most important that the laboratory building be reasonably remote from paved thoroughfares, from foundries and plants where heavy manufacturing is done, and that it be so constructed as to protect it, as far as may be, from temperature variations

This applies as well with respect to the sources of electrical and mechanical power for the laboratory itself, and to its machine shop, dynamos, air compressers, refrigerating machinery, to rooms for carrying on such special researches, themselves as require powerful machinery, steam pressure, gas furnaces, or anything by which the observatory building or other work in progress there, could be disturbed. These would, therefore, be best located in the general power house of the institution at some distance (200 feet or more) from the main laboratory.

There are two methods of attacking the problem of constant temperature, both of which are necessary to a successful result. The one involves the circulation of artificially cooled or heated air with the help of suitable regulating devices, the other that the building be constructed in such a way that outside temperature changes pro-

duce a minimum effect within. The first is accomplished by proper heaters, refrigerating machinery and automatic regulators. The second is a factor which must enter into the construction of the building from the outset and will add materially to its cost.

13. General character of laboratory building. — It is here that the experience of the Reichsanstalt is most valuable. The arrangement consists in a general way of central isolated rooms, one on each floor, with double doors, double walls and double glass floors above and below, thus giving inclosed air spaces on all sides of each room. Around this isolated room on each floor is a corridor. Neither the central rooms nor the corridor are connected with the heating plant of the building. Opening outward from this corridor is a series of rooms for general work, extending completely around the building. These are heated and ventilated from the plant mentioned above, which should be able to maintain the temperature constant within four or five centrigrade degrees throughout the year. Beyond these rooms is the outside wall of the building, of considerable thickness, and rendered insulating by means of perforated or porous brick, mineral wool or other suitable material.

It is plain that this construction must furnish the most perfect control of the temperature conditions which is possible in a building where many men are at work :—an outside insulating wall, the general laboratory rooms where the temperature is maintained constant by the circulation of air of constant temperature, a corridor which is really exposed to no temperature change, and finally, again inclosed within double insulating walls, an innermost room where the most refined experiments can be conducted without danger of temperature disturbance other than that from the body of the observer against which special provision is necessary to fit the conditions which obtain in each case.

Protection from the heat of the sun on the top of the building is secured by following the same general plan. So much of the roof as covers rooms lighted by side windows, i. e., the general laboratory rooms above described, would require to be double, and contain a thick layer of insulating material. The central rooms are dependent on overhead light, and the roof immediately over them must therefore be of glass, also double, and protected from the direct sunlight by a metal or tile roof, raised three or four feet above the glass to admit the light with the minimum of heat, somewhat as indicated in the accompanying sketch.*

* Here omitted.

A sub-basement, in which air circulates at a fairly uniform temperature, is also essential to dryness and constant temperature in the main basement where the larger permanent apparatus, for which stability is essential, is mounted. Within this sub-basement, inclosed by properly insulating walls, one or two underground chambers would be provided for such secular experiments as require constant conditions for longer periods of time, like diffusion experiments in viscous media. Such rooms require to be visited by the observer only at long intervals.

Vertical shafts for experimental purposes and special ventilation could be provided by towers at the corners of the building. Ordinary ventilation, hood flues, etc., would be carried by the outside walls, so far as practicable.

14. Floor space required.—A building of this character, to meet the needs of the work contemplated, would contain three (3) working floors above the subbasement. The first, or main basement, would contain the stone piers and would mount all apparatus requiring great stability. The next, or main floor, would contain the library, director's room, and general laboratory rooms; the upper floor the chemical laboratary, photographic laboratory, and work rooms for such apparatus as does not require extreme stability. At least a part of the roof should be available for mounting special apparatus out of doors.

This building would cover some 12,000 feet of ground over all, which, taking the distribution of space followed in the Reichsanstalt, would give a net floor space, exclusive of halls and corridors, of some 16,000 feet, of which perhaps 4,000 would be taken up with library, store rooms, balance room, clock room, mercury room, toilet rooms, offices, etc., which would be used by the entire staff in common, leaving 12,000 feet to be placed at the disposal of the director and the five investigators, and for reserve space in anticipation of future need. Approximately 1,500 feet would therefore be available for each investigator, with his assistant and mechanical helpers, for all the problems upon which he might be engaged, with 3,000 feet in reserve.

15. Special construction of building.—One other feature of the Reichsanstalt has proved of especial value, viz: The arched construction, which is maintained throughout the building, and enables the partitions between adjoining laboratory rooms to be taken away and two or more rooms thrown into one to meet the requirements of a particular experiment. Each arch section, or smallest

contemplated work room, contains complete heat, gas, water and electrical connections, and is thus potentially independent.

16. Necessity for special power house.—The needs of the laboratory in the matter of electric and other power will require careful consideration before the final plans are prepared. Electricity for charging storage batteries may be taken from any direct current supply wires of proper voltage, i. e., preferably 110 or 120 volts. Higher voltages involve special insulation and a great waste of power, unless an unusual number of cells are arranged to be charged simultaneously, which would be most inconvenient in a laboratory of this character. Street car feeds are not suitable for such a purpose; the voltage is high (usually 500 volts) and widely variable with the amount of traffic. Furthermore, if the distance from the power house were large, no considerable supply could be furnished to the laboratory without increasing the size of the feeds—an item of unnecessarily large expense—or interfering with the supply for the cars, a possible source of dissatisfaction, both on the part of the railway company and the laboratory.

Alternating current cannot be stored as such, and is, therefore, best produced by special dynamos under the control of the institution; the variations in a general supply main would be most disturbing. An exception might be made in the case of very high potentials, where the cost of a special wire from the power plant would be small, and the current could then be controlled by suitable transformers in the laboratory.

It will certainly be more satisfactory and probably cheaper for the institution to control its own electrical plant, especially as both alternating and direct current is likely to be needed, and at varying voltages, which would hardly be obtainable from outside.

17. Summary of Part II—The general requirements may now be summed up as follows:

I. A site, offering—
   (1) A rock foundation.
   (2) Permanent freedom from mechanical and electrical disturbance.
   (3) Sufficient elevation to insure dryness and good light.

II. A two-story building with basement covering approximately 12,000 feet of ground, and offering—
   (1) A stable foundation for apparatus to which extreme stability is essential.
   (2) A thoroughly insulated construction for controlling the temperature conditions within.

III. A power house at some distance (at least 200 feet) from the laboratory, to contain the machine shop, steam power, dynamos, storage batteries, the heating and refrigerating plants, rooms for special researches likely to involve tremors or electrical disturbances.

APPENDIX 2 TO REPORT OF ADVISORY COMMITTEE ON GEOPHYSICS.

LETTERS FROM EUROPEAN SCIENTISTS RELATIVE TO RESEARCH IN GEOPHYSICS.

CONTENTS.

In response to a letter of inquiry from the Secretary of the Committee on Geophysics, a number of European scientists kindly gave their views on a laboratory for geophysical research.

[*Prof. H. Poincaré to Mr. Walcott, May 27, 1902.*]

[Translation.]

SORBORNE, PARIS, FRANCE, *May 27, 1902.*

DEAR SIR:

There is no doubt that advances in the physics of the globe will be of the highest interest, as well from a theoretical as from a practical point of view. I believe that useful work could be done in this direction, and with a prospect of success, if a man competent in such matters would undertake the direction of the work, and were to have at his disposition sufficient resources and a sufficietn number of assistants.

Please accept, my dear sir, the assurance of my distinguished esteem.

H. POINCARÉ.

To C. D. WALCOTT,
Secretary Carnegie Institution, Washington, D. C.

[*Lord Kelvin to Mr. Walcott, June 2, 1902.*]

15, EATON PLACE,
LONDON, S. W., *June 2, 1902.*

DEAR MR. WALCOTT:

I was sorry not to have time to answer your letter of April 24, regarding the proposed establishment of a geophysical laboratory, before I left America. I had not forgotten it, and was on the point of writing to you when your letter of May 15, connected with the same subject, reached me here.

I am very glad to hear that there is a prospect of a geophysical laboratory being founded as part of the Carnegie Institution of Washington. I think it may prove most beneficial. Observations of volcanic phenomena in all parts of the world might, I think, be largely promoted by such an institution. Since I had the pleasure of meeting you in Washington we have had a sad and terrible demonstration in Martinique and St. Vincent of the great human interests concerned, which should prove a great impulse to prosecuting the natural history study of the subject.

I suppose you know of Milne's seismographic work, which he commenced in Japan and is now continuing in the Isle of Wight. Such work might very properly be taken in hand by the Carnegie geophysical laboratory. Professor T. Gray, Rose Polytechnic Institute, Terre Haute, Indiana, would, I am sure, be able and pleased to give you good advice on the subject. He worked in conjunction with Milne on it in Japan.

In respect to purely laboratory work the melting temperatures of all kinds of natural rocks is a very important subject for experiment. So is the relative density of melted rock and of solid rock at the same or at slightly lower temperature. This could be done most easily by melting a considerable quantity of rock in a crucible and dropping into it pieces of the same rock solid, but previously heated up to nearly the same temperature. It should not be a difficult observation to find whether they sink or swim; except in cases in which the melted rock is frothy at the surface by emission of gas.

By book post I send you with this an article on The Age of the Earth, which you may have seen already, but probably without the addendum at the end, which bears on geophysical laboratory work. You will also see on page 75 of the print a reference to good work by Dr. Carl Barus in Mr. Clemence King's geophysical laboratory.

We have all been very sorry that circumstances did not allow the continuation and extension of that laboratory, and I am sure it will be a good thing if it can be followed up in the manner you propose. I cannot but think that Mr. Carnegie himself will be interested in the subject.

In answer to your letter of May 15, I am sorry I do not know enough to allow me to recommend anyone as specially well qualified for conducting the work.

Yours sincerely,

KELVIN.

[*E. Suess to Mr. Walcott, June 7, 1902.*]

VIENNA, *June 7, 1902.*

DEAR SIR:

Your very kind letter, dated May 5, has occupied me very much during the last days. There is danger in proposing experiments which must be conducted under conditions too different from those of nature. I hope I have not gone beyond your intentions in inviting two distinguished friends, one our ex-professor of physics, Mr. Mach, and the other our professor of petrography, Professor Becke, to a confidential discussion of your question. Experiments are now being made on melting points at Gratz (Doelter) and at Geneva (Brin), and are in preparation here. You know the brilliant experiments on cooling by Sir Robert Austen. But it seemed to my friends as well as to myself that our knowledge is singularly deficient in regard to the influence of pressure on the melting point, as well as on increase or decrease of solubility. For the sake of attaining exactitude of expression, I asked my friends to give me their opinions in a few written lines, and these I beg to append to this letter.

But I must confess that quite a flood of different pieces of work and questions of the most varied character arise before my mind. I beg permission to speak of one of these tasks.

It is a very curious fact, that any brick or piece of pottery during baking attains, and then retains for all time, the magnetic orientation of the place of baking. Every fragment of brick or of old pottery gives the magnetic meridian of the place and time of baking, not the declination, because the situation during baking is not known. The variation of the magnetic meridian during the last thirty or forty years can easily be read from our bricks, and if the

laws of magnetic variation were more accurately known, we should possess a curious new chronological method. French naturalists have begun to examine baked clays from the lava fields of the Auvergne, and in this case declination, as well as magnetic meridian, can be examined.

These French observations, contained in the Comptes Rendus for 1891 are a small beginning of a great task. This task is rendered somewhat more difficult by the circumstance that in taking samples of baked clays their present geographical position, say beneath a coulee of lava, cannot be accurately fixed by compass, the needle being disturbed by surrounding masses, so that true north in every case must be fixed by simple astronomical methods. It is quite superfluous to say that intense interest would be awakened by the examination of such baked clays from high latitudes, say, from the basaltic region of Disko, or any boreal district. All questions relating to a variability in the position of the poles, etc., are thus raised. So, too, a series of observations from superposed beds of one and the same region, for example the Cascades, would be highly instructive, and a series of observations from the West Indies or Mexico to boreal districts would be highly meritorious work, and, so far as I can see, within the scope of your institution. Perhaps this may in future prove to be the way to attain a positive chronology for certain geological events, but the seemingly periodic character of magnetic variation will, I fear, turn out a difficulty which only the coming generation may overcome.

I remain, dear sir, respectfully, and full of sincere envy,

Yours,

E. SUESS.

[*Dr. Ernst Mach to E. Suess, May 29, 1902.*]

VIENNA, *May 29, 1902.*

MY DEAR MR. PRESIDENT:

I transmit herewith the letter which I was unable to give you yesterday, and also a few lines on the question there touched upon:

How can Geophysics best be promoted? The question is easier put than answered. It cannot be denied, however, that the determination of the effects which the highest attainable pressures, as well as the highest and lowest attainable temperatures, exert upon the melting point or the freezing point, the latent heat, the specific

heat, the conductivity, viscosity, and other physical constants of the terrestrial components would greatly advance geophysics. Mr. S. Guenther, professor of geography at the Technical High School in Munich, who has done much work in geophysics, might be more familiar with these questions.

With expressions of sincerest respect, your most obedient,

DR. ERNST MACH,

[*F. Becke to E. Suess, June 6, 1902.*]

VIENNA, AUSTRIA, *June 6, 1902.*

MY DEAR MR. PRESIDENT:

In accordance with your wish I take the liberty of communicating to you the following project for experiments bearing upon the development of slaty structure in crystalline schists.

According to a law announced by Riecke in Goettingen early in the nineties, the melting point is raised by mechanical deformation, or what is the same thing, the solubility of a body in the surrounding solution is promoted so that if in a saturated solution two bodies of the same kind as that dissolved exist, of which one is subjected to mechanical pressure (or is stretched or twisted), while the other remains in a state of ease, the strained body is dissolved, while the unstrained body grows at the expense of the same solution.

This law may be applied to an aggregate of crystals surrounded by a saturated solution of their substance. If the aggregate is subjected to a one-sided pressure, the portions under pressure should grow. The crystals should flatten perpendicularly to the direction of pressure, and a slaty structure should result.

Thus one might make a cake of salt soluble in water, such as alum, epsom salts, etc., and subject it to a one-sided pressure in such a manner as to permit of lateral yielding, the vessel meantime being filled with a saturated solution of the salt.

It would be particularly interesting to investigate the influence which especially favorable directions of growth might exert; how, for example, on the one hand isometric crystals would behave and, on the other hand, such as have a natural tendency to a tabular or prismatic form.

With the highest respect, your obedient,

F. BECKE.

[*Dr. O. Kohlrausch to Mr. Walcott, June 15, 1902.*]

[Translation.]

CHARLOTTENBURG, *June 15, 1902.*

HONORED SIR:

Although your inquiry is directed to the physicist rather than to the geologist, a competent judgment from the physical standpoint will require at least a superficial knowledge of geological questions and of the geological point of view. For this reason my answer has been delayed.

With the help of the literature and from conversation and correspondence with colleagues, I think I am now sufficiently informed upon the question to be able to say without reservation that a physical laboratory for geological purposes may achieve great success. So far as known to me such an institution would be among those laboratories which at present are subject to no competition. This is an important point. For if such amply endowed institutions are to be created for physical research, as are now contemplated in various parts of the world, an effort should be made to distribute the problems among them so far as it is possible. Each nation will, of course, reserve to itself many fields of scientific investigation, as well as the application of the results, whether similar investigations are in progress elsewhere or not; I might name as such, for example, thermal, electrical and optical tests of instruments and materials in addition to the more general scientific investigations. But there is such a strong tendency continually to subdivide the field of scientific research, and each subdivision when undertaken upon a large scale requires so large an expenditure of funds and labor, that economic considerations make it desirable to divide up the task in undertaking the various special fields of research. The results of such investigation, of course, become international property and duplication is seldom necessary.

For this reason it seems to me an important argument for the founding of a geophysical laboratory within the Carnegie institution, that there is not now in existence an institute with large resources, dedicated to this purpose.

It is furthermore a conspicuous fact that the United States, by reason of its extended and widely varied natural resources, would offer an especially favorable opportunity for a geophysical institute.

The chief problem of the institution will be to establish the con-

ditions under which the component parts of the earth form and have been formed. High temperatures and pressures will therefore be called upon at once. Physics is, no doubt, still far removed from meeting all the needs of geology in this direction. Even though 3,000 atmospheres and 3,000 degress Celsius represent imposing figures, and these magnitudes not only can be reached, but are already or in the immediate future will be measurable, in the *combination* of pressure and temperature, we must be content at first with more modest dimensions. But we shall certainly be indebted for advances in these problems to just the very stimulus arising from geophysical investigations. Electrical methods of heating and of temperature measurements are of very recent date and are still capable of much further extension. Bolometrical, and especially optical methods of pyrometry also promise important developments.

But even with the present limitations elasticity and rigidity, plasticity, melting points and their dependence upon psessure, vapor tension, critical temperature and the chemical relations, especially to water and carbonic acid, offer fields of apparently unlimited scope where as yet little has been done. Mutual solubility and crystallization form solutions, i. e., the separation of magmas into their component parts, are already within the reach of comparatively simple resources, and will certainly lead to most important results.

Electrolysis, which, having been cumulatively in operation for thousands of years, and must have been an effective force in the formation of the earth, still remains an entirely virgin field from a geological standpoint. Thermoelectric and other electrical potentials and the currents developed in the body of the earth by them, can scarcely be said to have been studied with reference to geological questions.

Magnetism, the measure of the earth's attraction, glaciers, and seismometry are probably already provided for in existing bureaus of the United States Government, and consequently need not be drawn upon to furnish a field for a geophysical institute.

For these reasons I must, therefore, agree entirely with your opinion, sir, that:

"The time seems ripe for the attempt and it would appear that success must be attended by most notable contributions to pure science, as well as to the history of this planet."

With the highest respect,

O. Kohlrausch,
President, Physikalisch-technische Reichsanstalt.

[*J. H. van't Hoff to Mr. Walcott, June 22, 1902.*]

CHARLOTTENBURG, *June 22, 1902.*

DEAR SIR:

In reply to your letter of May 5, wherein I am honored by your consulting me on plans for research in geophysics, I express as my conviction that an investigation in that direction might prove of the highest value, if made in a systematic way and continued for some years.

The special problem, which, I mean, deserves attention, is the physical chemistry of high temperatures applied to the chief constituents of the earth's crust, silicates in the first instance. To express myself more clearly, I add that two great problems concerning geophysics may in the present state of our knowledge be solved, viz., the evaporation of complex solutions, which have produced systemic deposits, such as salt layers, etc.; and secondly, the cooling down of molten masses, that have produced the volcanic and plutonic formations.

With the first problem, by far the easier one as regards apparatus, etc., I have been occupied for more than six years, and a series of twenty-six publications in the Annals of the Prussian Academy of Sciences (1897–1902) shows how far these researches have been carried out.

By the same post I send two abstracts, one by Armstsong, the other forming part of the lectures I delivered at Chicago last summer. It is my opinion that, guided by the indications obtained, the second problem, concerning the formation of plutonic and volcanic products, may be successfully taken up, but to pursue it in a systematic way the co-operation of different forces, furnished with special facilities for research extending over some years, is needed, such as can only be realized by an Institution like that newly founded by Mr. Carnegie. I may add, however, in favor of the project, that, when once an installation for high temperature research, with the special aim pointed out, has been established, many problems of the highest importance might be successfully studied with the same means. I suggest, moreover, that the use of Niagara Falls as a source of electric heat for the above purpose be taken into consideration.

Respectfully yours,

J. H. VAN'T HOFF.

[*Prof. G. H. Darwin to Geo. F. Becker, June 26, 1902.*]

NEWNHAM GRANGE,
CAMBRIDGE, ENGLAND, *June 26, 1902.*

MY DEAR SIR:

It is clear that there is a very wide scope for good work in geophysics in all the directions which you specify in your memorandum, and, as I said to Mr. Walcott, the limitations are set rather by the men than by the subjects.

I do not know the procedure by which the Carnegie trustees will allot the money to various projects. It may be necessary to draw up a scheme, complete in all lines of research. Mistakes will inevitably be made, and all that can be done to avoid them is to take great pains in drawing up the proposals.

I believe, however, that the most efficient plan would be to make the start in a humbler scale, but in such a way as will easily allow of expansion in various directions. This conception would only be best if money will be forthcoming for expansions when they shall be seen to be desirable and feasible. My reasons for saying this is that a geophysical observatory and laboratory is a new thing, and can not be planned with the same completeness as is possible in the case of astronomy.

Whatever line is taken it is very desirable that you should have thorough knowledge of the methods pursued at Strassburg, Göttingen, the Italian and Japanese observatories. I would recommend that you should learn what Milne is doing in the Isle of Wight, and hear what he has to say as to equipment. I believe that Urechert's great pendulum at Göttingen is better than any other instrument of the kind for a fixed permanent observatory, but it must have been so expensive as to be beyond the means of almost any private person.

I venture to suggest two researches which would, I think, be of interest. I should like to see made a study of earth tremors and deflections of the vertical deep down in mines. I conjecture that it would be necessary to install two, or even three, instruments at the top and bottom, and, perhaps, at an intermediate depth. If this matter has been studied at all, at least it has been very imperfectly investigated. Might it not perhaps throw some light on the broad yielding of the solid earth?

The expense would no doubt be considerable, and the observer must be a competent man who can make daily visits to the bottom.

I do not think we yet know sufficiently how far neighboring instruments give consistent readings even to the horizontal plane, still less the nature of the differences in the vertical line. There is always a doubt as to the proportion of the observed deflections which are due to mere local warping of ths soil and building. For example, it seemed useless for my brother and me to go on with our pendulum observations here, when a large part of what we noted was probably merely due to variations of water level in the river gravel. Consistent readings from two instruments several miles apart on the chalk hills would have had a very scientific value.

If, however, we wish for example to study changes in the vertical, to prove the existence or nonexistence of tremors due let me say to distant volcanic explosions, etc., we require platforms not affected by changes in temperature, underground moisture, and beyond the range of artificially produced vibrations. Stations complying with conditions such as these are rare. But would it be wise to build and equip an institution in a proper locality before preliminary investigations showed the realty of the phenomena to be investigated?

Then, again, there are so many researches where Mahomet must go to the mountain. Take our seismic survey: With my stable as a laboratory and the co-operation of thirty-six stations distributed over the world, you know the results we are obtaining respecting the physical nature of our planet, the districts which are yielding in its crust, etc. Strassburg with its Government support and a laboratory I envy, is without this outside co-operation, with the result that it can do but little more than publish its own registers.

Again, if we wish to make observations on seismic disturbances or changes in the vertical underground, we are again outside our four walls. To measure the effects of tidal loads on coast lines—the effects of barometrical pressures—secular deformations in the crust of the earth—the variations in magnetic elements, or changes in *g*; say in the vicinity of extinct volcanoes, we may be 1,000 miles away from our laboratory. And so I might continue suggesting lines of research, none of which could be carried out in a particular building. To my mind, what is required is a trust for geophysical work. If the funds admit, let there be a central office laboratory and staff, but the chief expenditures should be for investigation, carried out in suitable localities or at existing establishments. * * *

The enclosed pamphlets will show Mr. Walcott what I do during the year. In addition there is a heavy correspondence with stations and the instructors at Shide to be looked after.

This work costs about £150 a year * * * ."

The other subject which I suggest is a study of the actual motion in geological faults. My brother Horace has recently begun observations at a well-known fault in Dorsetshire. He has a very delicate level clamped to the rocks on the two sides of the fault and has begun his readings. Nothing has as yet been published,* as he has met with many preliminary troubles, but I think that the results should be of interest even if they are purely negative. I am sure that he would be willing to put his experience at your disposal.

To initiate a geophysical observatory even the humbler lines that I advocate for a beginning will undoubtedly prove a very arduous undertaking. If the work is entrusted to you I am sure that your great geological experience, and all the thought which you have devoted to geophysics will prove invaluable.

I look forward with the greatest interest to future developments and earnestly hope that the project will meet with the approval of the trustees.

May I ask you to show this to Mr. Walcott, who has just written to me in the same sense as you.

I remain yours very sincerely,

G. H. DARWIN.

G. F. BECKER.

P. S.—After writing the above I thought I had better consult Milne. He writes: "Very much geophysical work may be done in a laboratory, but it must not be overlooked that there is very much that can be done outside the same. Many of the hitherto suggested investigations, as for example, those relating to high temperature phenomena, might be carried out at existing laboratories, provided they had the means and the men."

[*Dr. W. Nernst to Mr. Walcott, August 26, 1902.*]

[Translation.]

GÖTTINGEN, *August 26, 1902.*

TO THE SECRETARY OF THE CARNEGIE INSTITUTION,
Washington, D. C.

DEAR SIR: I was unable to give an immediate answer to your esteemed letter of May 5 of this year, because, owing to pressure of

* See p 119, B. A. Rept., 1900.

work during term time, I had no opportunity to occupy myself with the matter in such detail as its importance deserved.

I fully agree with you in holding that the physics of the higher temperatures in particular is a field of the greatest importance for the theory of all physical and chemical processes on the one hand, and, on the other hand, particularly apt to throw new light on many questions of geophysics. Furthermore, we are here dealing with a subject which can hardly be dealt with by means of the ordinary resources of laboratories, because it demands special appliances. Of course it must be remembered that these exceptional requirements are demanded not only of the instrumental equipment, but also of the experimenter himself; in other words, a notable result can only be hoped for if the right men are found for the execution of this difficult work. Fortunately, your country possesses such investigators; in particular, you have in Professor Barus one of the foremost authorities in the field of high temperatures.

Having endeavored to answer your special inquiry, allow me, Dear Sir, to add a few remarks of a more general nature which forced themselves on my mind while reflecting on the magnificent institution which you are about to create. It seems to me that the intentions of the generous founder might, perhaps, be most fittingly carried out by the creation of an *academy*, organized on the whole in a manner similar to the academies of the Old World, but yet, in view of the abundant means at its disposal, differing from our academies in one essential point. The members of our academies, such as those of Paris, Berlin, Vienna, Göttingen, etc., are academicians only incidentally. The academic position is solely an honorary office, and in many cases hardly more than a mere decoration. Your institution, it seems to me, would be in a position to establish an academy constituted of investigators of the first rank, who would be academicians and nothing else; that is to say, they would be so placed as to live exclusively for the interest of scientific investigation. My idea is that a small circle of the most eminent investigators might gather in Washington, composed of those whose method requires a scientific laboratory, with a small outfit appropriate to their special mode of investigation. It may be remarked that the cost of these laboratories would not be great compared to the corresponding laboratories of universities, because in the latter the larger part of the funds is devoted to instruction, not to investigation, and for the same reason the proposed laboratories might for

the most part be of much smaller dimensions than laboratories for instruction. This would also be an advantage inasmuch as the efficiency of the experimenter is by no means always in proportion to the size of his laboratory. In my opinion, small laboratories, but with first class outfit, should be the aim of every investigator.

The men for such an academy could be found in your country at once in most branches, and in all branches in the near future, since your country is progressing in science at a rate hardly equaled elsewhere.

I fear these remarks far exceed the scope of the question which you addressed me. It is needless to say that I shall take pleasure in giving any further information that you may desire.

Very respectfully,

DR. W. NERNST.

REPORT OF ADVISORY COMMITTEE ON GEOGRAPHY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The subject of geography being named without limitation in my appointment as adviser, I wish briefly to set forth the contents of the subject as a whole, in order to indicate the relation of its several parts and to point out those that seem most in need of aid.

Geography is best defined as the study of the relationship between the factors of physical environment (causes) and the responses of the environed organisms (effect), and hence of these factors and responses themselves. The subject may be treated systematically or regionally, but neither of these chief divisions has yet reached a mature development. Systematically, all the different categories of physical factors and of organic responses are taken up, illustrated by typical examples, and arranged in some appropriate order. Regionally, all the physical factors and organic responses occurring in a certain region are brought together, in an arrangement corresponding to that adopted for systematic geography. Under either of these headings special attention may be given to larger or smaller parts of the subject; but at present it is too generally the case that these partial studies are undertaken without any sufficient consideration of the relation of the parts of the whole.

The Carnegie Institution can do much to aid the development of the scientific and thorough study of geography, and some practical suggestions to this end are given below; but it is desired to explain at the outset that some of the most promising results may be expected from comparatively young geographers, for the reason of the undeveloped condition of the subject as a whole. In this respect geography occupies a very different position from astronomy. Geography has long had the service of men of energy and of action; but the more philosophical side of the subject has been little developed by geographers. Astronomy, on the other hand, has for centuries commanded the services of the greatest intellects of the world, and it today is recognized as holding a leading place among the sciences. It is in the hope of developing the more scientific or philosophical side of geography that I recommend below the granting of aid not only to professional geographers of established position, but also to

students of promise; and I should greatly regret it if the undeveloped position of scientific geography resulted in its being set aside for the support of other subjects already so well endowed that large and influential committees of eminent experts are easily organized in their advocacy.

The two chief divisions of geography may be further subdivided. Physiography includes all the facts of physical environment of life on the earth; and the other half of the subject—unnamed, but fairly designated by such a term as ontography—includes all the kinds of responses of physically environed organisms. Physiography is further divided into the earth as a globe, the atmosphere, the oceans, and the lands; ontography may also certainly be subdivided, but no divisions are at present agreed upon among geographers.

The Earth as a Globe.—All problems concerned with the size and form of the earth are best left with governmental surveys. If particular problems arise in this connection, such, for example, as local magnetic surveys, they will be duly considered when applications are made by specialists. For the present, no recommendation is made under this heading.

The Atmosphere.—A. There is much need of a scientific study of the failures of weather prediction. Within the last month there have been two examples of failures of precisely the same class as those of twenty years ago; and yet it does not appear that serious research is in progress with a view to lessening such failures. Much might be done by a competent investigator who should review maps and predictions and classify successes and failures.

B. Another line of work toward the same end is a study of the movement of the upper clouds by means of the horizontal mirror. A study of this kind might be conducted at a moderate expense and useful results could be expected in a year or two.

C. Detail of weather phenomena. The meshes of the Weather Bureau net are so coarse that many smaller phenomena slip through them undetected. It has frequently been suggested that a scheme of observation by numerous voluntary observers, coördinated under state weather services, might be organized, so as to obtain a closer view of passing changes. The work done in this direction some fifteen years ago by the New England Meteorological Society, and continued for a season or two elsewhere, is well worth systematic extension over a larger area. It is to be expected that the Weather Bureau would permit the use of franked envelopes in such work; there would be a possibly large expense in providing instruments

Printing would be no small item ; and discussion of results would be an important charge. At present, it may not be possible to secure just the right men for such an investigation ; but if any such should offer themselves, I should favor their being well supported.

D. Investigation of the upper atmosphere by kites and balloons, kites preferred. While this kind of work is not likely to be for the present immediately useful in weather prediction, the study of the processes of the upper atmosphere is essential to reaching a full understanding of meteorological phenomena, and should be warmly encouraged. The Carnegie Institution might advisedly support the work of two qualified observers, providing them with an outfit of kites, instruments, etc., and allowing them to move from station to station, spending a year or more at each place. I urge that Mr. A. Lawrence Rotch, Blue Hill, Mass., as well as the Chief of the Weather Bureau, be consulted on this work.

E. Kite-flying from mountain tops deserves mention apart, as it would provide information concerning unexplored atmospheric regions. Work of this kind in connection with the Harvard Observatory at Arequipa, Peru, would be particularly interesting.

F. Since the publication of Ferrel's works there has been little mathematical or physical study of meteorological phenomena in this country. It is very desirable to enlist the interest of competent mathematicians and physicists in such work, and if any promising investigator comes forward he should be encouraged.

The Ocean.—Charting and sounding may well be left to governmental expeditions. Tides are also well cared for, being of such practical importance that they command abundant support ; but the Carnegie Institution might offer aid in supplying special instruments to exploring parties ; for example, sounding apparatus to some yachtsman who should undertake minute soundings of the submerged channels off our Atlantic and Pacific coasts—a very interesting and little-studied field. The actual movement of deep ocean waters should be determined, and ingenious instruments might be constructed for this purpose. Such studies well deserve aid.

The Lands.—A. The expenses of exploring expeditions had best be left, as a rule, to governmental and individual funds. There is no region, except one, whose exploration promises to produce new classes of facts of sufficient importance to call for large support ; but it would be well to offer aid to well-trained explorers by supplying them with instruments, after the fashion of the Royal Geographical Society. Sextants, barometers, thermometers, plane tables, cam-

eras, phonographs, etc., as well as apparatus for collecting and cases for shipping natural history specimens, would be of great service. The announcement that the Carnegie Institution stands ready to receive applications for aid from travelers properly trained for their work would, I believe, greatly promote the development of scientific travel among our young men. I would, however, urge that emphasis be laid on "proper preparation" in order to distinguish scientific work in some one of the various phases of geography from mere traveling and big-game shooting.

B. The exception above noted is in the Antarctic regions. New classes of facts may be expected from the study of far-southern latitudes. * * *

C. There are various special topics in which a trained investigator is sure of interesting results. For example (as suggested by Mr. Gannett), a sum might well be allotted in aid of studies of North American glaciers. Systematic work on glaciers should be inaugurated, to be continued for a long period of years. A moderate sum would be of much service in enlisting the interest of travelers, sportsmen, and others, who could bring good results home by following systematic instructions. The special studies of shore lines need extension in the field. Similarly studies of Appalachian rivers offer results of value in several connections; but the most available men are already supported in such work by the United States Geological Survey. It should be noted that in this sort of work it is not necessary, not even desirable, to explore in the ordinary sense—that is, to go into previously unknown regions. There is abundant field for scientific geographical exploration east of the Mississippi, and still more west of it.

Probably the best way to encourage work of this kind would be to announce the establishment of five or ten fellowships in geography, of from $300 to $1,000 each, to be assigned to well-prepared students who wish to undertake investigations in some part of the broad geographical field. It would be my preference that the idea of relationship, indicated above, should be made a prominent part of all such studies, in order to bring forward that fundamental principle that underlies all true geographical study. The work might be systematic—that is, concerning some special subdivision of the subject, as glaciers or shorelines, above mentioned, or it might be regional, concerning all the geographical features, inorganic and organic, of a certain region, Pennsylvania, for example. A study of this kind, well conducted, would greatly enlighten the public, even the scien-

tific public, as to the true nature and content of geography. As has already been pointed out, the principles of geography are so little developed that it will be necessary to begin studies of this kind in relatively unadvanced stages, for the advanced stages are yet to be reached. Some of these studies might be well undertaken in the library, at least in part, for there is already on record a large amount of material whose full discussion and digestion still requires much patient labor.

Summary.—In view of the foregoing suggestions, I make the following recommendations:

A. Announce the establishment of ten fellowships of from $300 to $1,000 each, offered annually for aid of special investigations by well-qualified students. The subjects of these investigations might be taken from any one of the subdivisions of geography. Some of them are specified above.

B. It should be further announced that the Carnegie Institution stands ready to aid well-qualified travelers and explorers by lending them instruments, etc.

C. Subjects of investigation above noted, apart from those just referred to, should not be advertised. They are mentioned in this report in order to indicate the character of work worthy of support; but it seems best to wait till the right man comes forward before undertaking them.

*   *   *   *   *   *   *

Respectfully submitted.

W. M. DAVIS, *Chairman.*

HARVARD UNIVERSITY, *April 12, 1902.*

REPORT OF ADVISORY COMMITTEE ON METEOROLOGY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The problems of future research in meteorology can be best appreciated after considering the past history of our knowledge of this subject.

The ancient history of meteorology ends with the establishment of a system of observing stations in southern Europe in 1653 by the Grand Duke of Tuscany, These stations were supplied with thermometers, barometers, hygrometers and wind vanes. From that time until the establishment of the Mannheim society, about 1780, local climatology was the principal study, but that society organized a system that was intended to cover the world as far as possible, and stimulate the study of the atmosphere as a whole. The observations published by it afforded Brandes in 1820 an opportunity to chart the atmospheric conditions for Europe day by day; this was the beginning of the study of storms and local atmospheric movements in Europe. About the same time Redfield began collecting and collating the logs of vessels at sea, and laid the foundation of our knowledge of Atlantic ocean hurricanes. Very shortly after this, Espy began collecting and charting observations in the United States and Canada for the study of the characteristics of tornadoes and the general rains of this country. In 1867 Leverrier began publishing his charts of Europe, America and the Atlantic Ocean under the title of the International Atlas. In 1875 General A. J. Myer began the publication of the Signal Service Bulletin of International Meteorological Observations, with an accompanying atlas, showing the conditions over the whole northern hemisphere, for each day of the years 1875–1884; this was continued as a monthly summary until 1889. Up to this time observations had been restricted generally to the earth's surface and sea level, except for an occasional balloon ascension and the regular observations of the clouds. In 1790 Alexander Wilson at Edinburgh, and in 1882 E. D. Archibald at London, used kites to get temperatures and wind velocities at considerable altitudes. About 1885 Mr. William Eddy, who had become famous for the use of kites to carry heavy objects high in the air, was asked by me to turn his attention toward the application of the kite to meteorology by way of carrying up self-recording apparatus. Since that time this use of the kite has

been especially developed by Mr. Rotch at Blue Hill and Professor Marvin at the Weather Bureau, so that, finally, in 1899, Professor Willis L. Moore, as Chief of the Weather Bureau, was able to organize a system of seventeen kite stations and lay the foundation for compiling charts of temperature, pressure, and wind at an altitude of one mile above sea level. In 1897 Monsieur Leon Teisserenc de Bort began at Trappes, near Paris, to systematically send up small balloons filled with hydrogen, carrying self-registering apparatus to great heights, and this work is now carried on at ten or twelve stations in Europe, so that by means of simultaneous ascensions we are able to make maps of the condition of the atmosphere at an altitude of 10,000 or 15,000 meters above that portion of the world.

With regard to the ocean we understand that at the present time the Hydrographic Office of the U. S. Navy, the Seewarte at Hambnrg, and the Meteorological Office at London are compiling not only monthly and annual summaries for each square degree of the ocean surface, but also daily maps of the atmospheric condition over those special parts of the ocean from which a sufficient number of observations can be obtained. Daily maps of the Atlantic Ocean have been published by the British and Danish meteorological offices, and similar maps of the monsoon area by the Indian Government.

Although there may be approximately 25,000 meteorological stations on land at the present time, and as many more good observers at sea, yet a large part of both the land and water surfaces are still unrepresented by charts or observers. Consequently our knowledge of local climates is very unsatisfactory, and our knowledge of the local movements or the so-called physics of the atmosphere has only just begun to be developed.

Of course meteorological research must cover an extensive field, including all lands and seas and extending upward to an elevation in the atmosphere as yet undetermined, but beyond which we may speak of cosmic physics or the physics of the ether. It also includes the applications to the atmosphere of our knowledge of hydro-dynamics, thermo-dynamics, kinetic theories of diffusion, optics, electricity, all of which are generally included in the term physics of the atmosphere. The study of local and general climatology affords also a practical illustration of the mathematical laws of probability.

The applications of meteorology to the practical needs of mankind and the relations of meteorology to biology, geology and other

branches of science are numerous and important, but the researches along these lines are outside of the fundamental science of meteorology.

On account of these important applications every civilized nation maintains a meteorological office and service responding to the practical needs of the people. At the present time there are about thirty larger organizations of this kind and twenty smaller. The progress of the science of meteorology, as distinguished from its practical applications, is largely but by no means exclusively in the hands of these national organizations. This science has also been fostered by meteorological societies of which the most prominent are those of France, England, Scotland, the Mauritius, Austria and Germany, to say nothing of the societies of Mannheim and New England, now defunct. Nearly every general scientific society also encourages meteorology. The universities of the world have in some cases organized and given special attention to meteorology; notable among these are the professorships held by Kämtz, at Dorpat; Woeikof, at St. Petersburg; Schmid, at Jena; von Bezold, at Berlin; Hergesell, at Strasburg; Lamont, at Munich; Hann and Pernter, at Vienna; Mascart, Angot and Brillouin, at Paris; Wm. M. Davis and R. De C. Ward, at Harvard. In addition to these full professorships, there are many instructors and lecturers, repsenting minor courses, in which climatology is taught as a part of the education of a physician, an engineer or a geologist.

The number of students who take the higher meteorological courses, and especially those who make meteorology the major subject for the attainment of the degree of Ph. D., is very small; apparently it does not average more than five per annum for the whole world. This condition of affairs is quite remarkable in consideration of the great importance of the science, and is to be explained, partly, by reason of the difficulty of the subject, but principally by the fact that the needs of the meteorological services of the world have, as yet, not been properly made known to the universities and to those who provide for the support of the faculties. As a consequence the older and prominent professional meteorologists are those who originally made a special study of chemistry, or astronomy, physics, navigation, or engineering; this gives to each meteorologist a tendency to prosecute meteorological studies along certain pre-determined lines of thought. There are, however, a few cases in which most important work has been accomplished by those who have approached the subject from the point of view of math-

ematics and analytical mechanics, and it is along this line of investigation that we must expect the most important discoveries in the future. No matter how diligently we prosecute our observations and collect and study the observed data, yet it must always be true that the fundamental laws controlling atmospheric phenomena must be those of mechanics and must be investigated by those skilled in mathematics. Meteorology has attained a status analogous to that of astronomy in the century between Newton and La Place. It is ready to receive a new leader and is looking for him. A hundred experimentalists and thousands of observers are perfecting the date of observation, but the crying need is for one who shall elucidate our complex phenomena to the satisfaction of the students of mechanics.

In this search for men and the pre-eminent right man, meteorologists welcome the assistance of the Carnegie Institution, and your committee would respectfully submit the following general recommendations which will be supplemented by fuller details whenever called for :

1. Meteorology should be treated by you as a very broad subject, always embracing the atmosphere as a whole. The Institution may leave it to local observers to investigate the climatology of their respective localities, embracing only one-tenth of the surface of the whole globe; the remaining nine-tenths, including the polar regions and the high seas, are open to your investigation without exciting international jealousies or questions of propriety.

Meteorology should be treated as a part of terrestrial physics, the other branches of which will be terrestrial magnetism, oceanography, geology, vulcanology, seismology, and similar matters that affect the globe as a whole. If, as it is reasonable to hope, the Institution organizes a department of research in terrestrial physics, then we recommend that meteorology be given a prominent place or division therein, and that the division be conducted by three persons, namely, a mathematician, an experimental physicist, and a bibliographer.

2. It is reasonably certain that the young men, who from year to year attain the degree of Ph. D., do in some cases desire to devote themselves to meteorology, but at the present time so little inducement is offered to men of high scientific talents to devote themselves to this field of work that, as a matter of necessity, most of them seek employment elsewhere. The Carnegie Institution will do its best work for meteorology by securing the services of young men who have shown already a genius for investigation in mathe-

matics, mechanics, and physics. Such persons should be appointed Fellows in Meteorology, with salaries of from $1,500 to $3,000, depending on age and experience, renewable from time to time, as occasion may demand, and who shall devote their whole time to appropriate research. The general trend of their researches should be prescribed by the three older persons who conduct this division.

3. A laboratory arranged for general physical research should be at the disposal of the fellows in meteorology, but at other times a so-called meteorological observatory, or special laboratory, would be needed, built with a view to special investigation. In some cases the fellows would be obliged to occupy distant stations or to take ocean voyages or ærial voyages, or to make use of sounding balloons and kites. All this does not constitute a very expensive matter, as the physical laboratory must be provided for general physics, and the meteorological laboratory or observatory is quite a simple matter.

4. The research problems appropriate to the Carnegie Institution may in some cases seem also appropriate to the various government weather bureaus, but that is principally because such bureaus have not confined themselves to the practical applications of what is known in meteorology, but have also devoted a small portion of their attention to research in lines that promise to be helpful to the progress of their specific duties. Without encroaching upon the privileges and duties, or the fields of labor imposed upon these government bureaus, the Carnegie Institution may often prosecute studies along the same lines of inquiry, and, in fact, frequently this will be very desirable, especially when a given investigation requires the co-operation of more stations or more individuals than any one bureau can command. Several large works of this character have been suggested by Professor Willis L. Moore in his communication of April 15, as Chief of the United States Weather Bureau, and we repeat them herewith, after rearranging them in what we conceive to be the order of their importance to the immediate needs of meteorology. Whenever the Carnegie Institution intimates its desire and intention of taking up any one of these works, we have reason to believe that the Weather Bureau, the Hydrographic Office, and other similar institutions in Europe will co-operate most heartily. The list is as follows:

(a) General bibliography of meteorology up to 1900 inclusive.

(b) General meteorology of the upper atmosphere, to be studied by means of clouds, balloons, kites, mountain stations, polarization of skylight, or any other method of observing the upper air.

(c) General meteorology of those parts of the ocean not already provided for.

(d) Daily weather maps of the world in general, compiled by international co-operation, from reports received by mail from observations on land and at sea.

(e) The relations of meteorology to terrestrial magnetism, atmospheric electricity and solar radiation, including the absorption of sunshine by the air.

5. Among the minor subjects that may be taken up by single individuals in the physical laboratory or meteorological observatory are the following:

(f) All problems relating to thermometry, barometry, actinometry, anemometry, hygrometry, pluviometry, nephelometry and other branches of instrumental work. In every field of observation we need continuous self-registering apparatus more sensitive, more delicate and more reliable than we at present have.

(g) Experimental laboratory methods should be devised to elucidate the physical processes of the formation of cloud, fog, rain, dew, frost, snow and hail, which work will necessarily be a continuation of that already done by Carl Barus and C. T. R. Wilson, so far as concerns clouds and rain, and that done by Mr. W. A. Bentley, of Jericho, Vermont, as far as concerns the microphotographs of snow crystals.

6. But, as above said, all these observational researches must be supplemented by mathematical work on the dynamics of the earth's atmosphere. Only a few elementary problems under this category have as yet been solved satisfactorily, and perhaps those that remain cannot be solved until new branches of mathematical analysis shall have been developed for this purpose. It is in this line of work that we most earnestly anticipate the assistance of the Carnegie Institution. A few years ago meteorologists were encouraged to find that von Helmholtz had turned his attention in our direction. But his death in the prime of life crushed our hopes. At the present time there are several prominent workers on the mechanics of the atmosphere, such as Bigelow in America, Bjerknes and Ekholm in Sweden, Moeller, Sprung, Wien, Pockels and von Bezold in Germany, Marchi in Italy, Pernter and Margules in Austria, Diro Kitao in Japan, and Brillouin in Paris. These all combine a good practical

knowledge of weather maps and actual meteorology, with a knowledge of the present status of mathematics. But it is evident that mathematical analysis is as yet scarcely able to cope with the real problems of meteorology. These authors have therefore generally treated only the simplified problems idealized from nature, whereas apparently we need mathematicians of the highest genius, who shall devise new methods applicable to the complex conditions that control the atmosphere. It is such a mathematician as this that we had in mind in recommending that three persons, of whom the mathematician should be chief, be entrusted with the conduct of the division of meteorology.

Respectfully submitted,

CLEVELAND ABBE,
Chairman.

JULY 14, 1902.

MAJORITY REPORT OF ADVISORY COMMITTEE ON CHEMISTRY.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The Advisory Committee on Chemistry appointed by you, consisting of the undersigned, have given their earnest attention to the subject submitted to them. They have kept clearly in mind that the main object of the Institution is to encourage original research. The problem is to determine what methods of expending money will be most likely to accomplish this main object.

In the first place, it appears clear to your committee that, so far as Chemistry is concerned, the most valuable researches are to be looked for in the universities, colleges, and technical schools. While they are well aware that researches in the field of Chemistry are carried on in other laboratories than those of the universities,* they are of the opinion that the university atmosphere is the most favorable for such work, and, as a matter of fact, for many years past the advancement of Chemistry has been largely due to the work done in university laboratories. We must, then, look to the universities for the men and for the conditions if our object is to further original research. Anything that will tend to increase the efficiency of the men already engaged in research work will be helpful. This efficiency can be increased in two ways:

(1.) By relieving the men from a part of the routine work they are now doing.

(2.) By placing research assistants at their disposal, and by supplying them with books, apparatus, and material.

The committee agree that the second way would undoubtedly lead to good results if followed carefully. They do not agree that it would be wise to follow the first way, at least with the aid of funds furnished by the Carnegie Institution.

In accordance with these general ideas, the committee agree upon the following articles of belief:

1. The best research work is likely to be done under the auspices of the universities.

* In this report the term university is used in the larger sense, as including all educational institutions in which research work is carried on.

2. Some teaching is helpful, and therefore desirable, in connection with research work.

3. Those should be encouraged who have shown their power by independent research work and have shown persistence under difficulties.

4. One of the obstacles in the way of research work in Chemistry in this country is the large amount of routine work that some of our best men are required to do.

5. Good work is most likely to be done as a result of individual initiative.

We therefore recommend, as suited to the needs of chemical research, the establishment of a number of *Carnegie Research Assistantships*, the assistants to be appointed thus:

A number of chemists now carrying on research work, who have given clear evidence that they will continue to carry on such work, are to be selected as worthy of the aid of such assistantships. They are to have power to appoint their own assistants, under such conditious of time and compensation as may be acceptable to the Board of Trustees of the Carnegie Institution.

We recommend, further, that workers should, when necessary, be aided by appropriations for the purchase of apparatus, material, and books. In such cases the applicant should make a clear, but not necessarily detailed, statement in regard to the character of the work to be done and the kind of apparatus needed.

Respectfully submitted.

IRA REMSEN, *Chairman*,
T. W. RICHARDS,
E. F. SMITH,
Committee.

OCTOBER 14, 1902.

MINORITY REPORT OF ADVISORY COMMITTEE ON CHEMISTRY.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: While approving the plan recommended in the preceding report, I feel that it is only a partial remedy for the difficulties which retard American research. To me the above-named *first* method of increasing the efficiency of the men already engaged in research seems a more important means, namely, the plan *to relieve university professors from a part of the routine work which they are now doing.*

It is true that in a few great universities some at least of the professors are given time for research. These men are the chief examples of the value of the university atmosphere. But this freedom is the exception rather than the rule. Not only are most American professors overburdened with routine work, but nearly all are obliged, by the inadequacy of their salaries, to consume time and energy in hack work, done merely to obtain a competency. Neither money for assistants nor money for materials can penetrate to the root of this trouble. Such money might, indeed, prove but a temptation to overwork and consequent break-down.

It seems to me that *the difficulty can be most satisfactorily overcome by the establishment of Carnegie Research Professorships*, to be awarded to those whose past originality and persistence have shown that they deserve such positions. These men should be allowed to retain some teaching work in the university of their choice, and should be paid for such work by the university, but the bulk of their salary should come from the Carnegie Institution. This arrangement would cause each Foundation to pay for its appropriate work.

The appointments to these professorships might be made for a definite term of years, subject to reappointment in case the appointee has given evidence of due earnestness and success; or they might be terminated only by death or proved inefficiency. The total salary should be large enough to relieve the professor from pecuniary worry, and he, on the other hand, should promise to engage in no money making pursuit outside of the university. It would be well to provide each professorship with a Carnegie Research Assistantship.

The *nominations* to these professorships of chemistry might be made by a disinterested foreign committee of experts, such as J. H. van't Hoff, E. Fischer, W. Ostwald, M. Berthelot, and W. Ramsay; or else the candidates might be selected by the independent voting of a large number of the leading American chemists. Of course the *appointment* would rest with the Trustees.

This idea is not wholly new. The German Government has already begun to establish somewhat similar professorships, and unless America does likewise there is danger of our dropping yet farther behind.

It seems to me that the establishment of these professorships would not only benefit science through the appointees, but would also furnish an immense stimulus to the prosecution of research among younger men. At present an intelligent and far sighted man perceives that he cannot hope to provide comfortably for a family if he gives his chief energy to research. There is no prospect in that direction. The able teacher or administrative officer in a college may become president, the able inventor may secure a competency through his patents, but the pure investigator is doomed to poverty. There is no doubt that this lack of prospective advancement has driven many a brilliant American away from the vastly important field of activity which it is the office of the Carnegie Institution to foster.

Most respectfully submitted.

T. W. RICHARDS.

OCTOBER 14, 1902.

REPORT OF ADVISORY COMMITTEE ON ASTRONOMY

CONTENTS.

APPENDICES.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Out of the multitude of problems in general and in detail which your Committee in its advisory capacity has studied with care, the importance of certain general views concerning astronomical interests has impressed itself on our minds. We think we ought to state some of the most important of these at the outset, because of their general application to all recommendations which we, as advisers, desire to present for the consideration of the Carnegie Institution.

Support of Astronomy in the United States.

The first point (intimately connected with the second) is that the relatively small support which American astronomy receives from the general and state governments contributes, in our opinion, a strong argument for a more liberal support to astronomy on the part of the Carnegie Institution than might be properly urged were the circumstances otherwise. This applies both to the character and amount of support which is needed. The leading governments of Europe each maintain in whole, or in part, many astronomical observatories distributed at various places in their respective dominions. The government of the United States contributes to the support of a physical observatory in connection with the Smithsonian Institution. It also maintains what was once known as the National, and subsequently as the Naval Observatory, in charge of officers of the Navy. One of the objects of this institution is to make astronomical observations. The general government maintains no observatory outside of Washington. Several observatories have been established by state governments in connection with their respective universities, to which more specific allusion will presently be made.

Need for More Workers.

The second point to which we invite special attention is that the first need of astronomy in this country, as it seems to us, is not for more buildings and instruments so much as for more astronomical workers to use the appliances which are already provided. In the observatories which have been established with or without state aid, the liberality of private individuals in providing for buildings and equipment has had no parallel in any other country. We have nearly as many large telescopes as exist in all other countries of the world combined. Included among these are the telescopes at Yerkes Observatory (40-inch) and at Lick Observatory (36-inch), which are the largest in existence. On the other hand, permit us to call attention to the remarkable fact, that, with one exception among state observatories, and two exceptions among those established and supported by private endeavor, none is maintained with an income which is much larger than is necessary for the payment of incidental expenses, together with the salary of the professor in charge, whose teaching duties usually leave him little opportunity for the routine

part of research. Thus the American system is weak in its most vital part. Problems in great number are pressing for solution; there is already a generous provision of observatories and equipment inviting use; there is a body of skilled investigators who are anxious to make good use of these material facilities; but there is very marked deficiency in means for their support. By support is meant the necessary provision for assistants and computers and for other expenses incident to the maintenance of active research on important problems. In reference to almost all the important subjects of inquiry, research in astronomy is attended with the necessity for a very great amount of skilled labor in measurement and computations. The conception of new ideas in investigation, the formulation of plans and methods, invention of devices to improve and shorten labor, and the deduction of results, demand the same order of ability that is required everywhere in the direction of work in exact science; but usually in the details the same kind of measurement or computation has to be repeated in the same way thousands of times, sometimes on thousands of different objects, requiring for success the habit of accuracy, or manual dexterity, quickness and industry, rather than a high order of scientific ability. We believe that skilled computers and observers for the routine work are needed in greater proportion for the successful prosecution of research in astronomy than in any other department of exact science; and provision for these we believe to be the most pressing need of American astronomy at the present time.

Proposal for a Southern Observatory.

The third point which has specially impressed itself upon our attention is the great deficiency of observatories in the southern hemisphere. The chief contributions of observations upon the southern sky are now coming from four or five observatories only, in a less degree from three or four others. Against this we have in effective force about ten times as many working observatories in the northern hemisphere. Since more than one-quarter of the entire celestial sphere is efficiently reached only from the southern hemisphere, it is obvious that there is now very great disparity of astronomical resources to the disadvantage of the southern hemisphere. In studying this matter we have become more and more impressed with the idea that, if possible, something ought to be done to remedy this disparity. The scheme for an observatory in the southern

hemisphere presented in Appendix A may not be realized in full for many years to come, but we have thought that it may be possible to make the preliminary studies for its location and scope now, and we respectfully submit the question for the consideration of the Carnegie Institution whether it may not be also soon possible to make a modest beginning in the actual establishment of such an observatory. We regard this question to be exceeded in importance only by the urgent need of provision for current work to which we have already alluded.

Coöperation in Research.

We think that the Carnegie Institution might do well to encourage a greater degree of coöperation among astronomers, and we believe that its organization and scope would be well suited to this purpose. There are large fields of astronomical work involving masses of observations and computation which cannot be successfully dealt with by any single existing observatory. Numerous examples of effective coöperation upon extensive plans of observation are to be found in recent astronomical work. The catalogues of stars prepared by various observatories under the auspices of the Astronomische Gesellschaft represent a case of this kind. In order to produce the most valuable results and to escape the dead level of mediocrity, organization must leave room for the exercise of individual genius.

Problems for Research.

In the following suggestions regarding certain lines of research which we believe could be wisely aided by the Carnegie Institution, we have done little more than enumerate specific problems. For details we would especially refer to the appended individual reports of members of the Committee.

The work of astronomy may be broadly classed under two heads: (1) Investigations which involve the determination of the positions of the heavenly bodies and of their motions of every kind, real and apparent; (2) investigations on the physical and chemical constitution of celestial objects.

The Sidereal System.—Fundamental determinations of star positions lie at the foundation of the science. It is of the first importance to know the exact positions and motions of the brighter stars, since all other observations for the positions of celestial bodies are

based upon this knowledge. There is decided deficiency in determinations of the positions of the principal stars at the present time. This work is very exacting and laborious, and, in our opinion, is worthy of ample aid. The extension of such observations to the southern hemisphere deserves the careful attention of the Institution.

Much work still remains to be done in the deduction upon modern principles of revised results from the older series of observations. This work is of very great importance and is worthy of support.

Measurement of the parallaxes of stars to ascertain their distances from the earth should be undertaken with powerful instrumental means on broader lines than those hitherto followed. On the results of the investigations mentioned in what precedes, combined with those derived from the spectroscopic measurement of stellar motions in the line of sight, and the determination of the positions of nebulæ, the solution of the great problem of the structure of the universe must be based.

The accuracy of star positions is connected with an exact knowledge of the constant of aberration, which demands further study by new and more perfect methods. This constant has also a physical interest of its own. Equally desirable is more precise information regarding the changes in the direction of the earth's axis of rotation, rendered possible through observations of the variation of latitude.

Many problems relating to the stellar system call for research. The existing uncertainty regarding the motion of the solar system in space should be remedied through a more comprehensive investigation than has yet been attempted, an undertaking which is now especially timely and feasible.

The Solar System.—The lunar theory requires special attention. Tables in general use are based upon Hansen's investigations published forty-five years ago, and need thorough reconstruction in order to meet the present requirements of astronomy. Of fundamental interest is the question whether Newton's law of attraction, according to inverse squares, is exactly true within the limits of error of observation. It is desirable that this problem should receive further investigation. * * *

The minor planets, nearly 500 of which are known, present a problem of much difficulty, which demands attention. Some provision should be made for more numerous computations of their orbits. This subject can be effectively handled by means of well-devised coöperation, to which it is very well adapted.

The computation of the solar parallax from observations of the

minor planet, Eros, is a subject of interest at present. A vast accumulation of photographs, together with some micrometric measurements, has been secured. From lack of funds or other causes, little progress appears to have been made in rendering the results available to science. What share in this work, if any, should be undertaken by the Carnegie Institution is a matter for future consideration, after a better knowledge of the situation may have been obtained.

Publication of Results.—There is reason to believe that there are in existence valuable series of observations, calculations, and compilations of great interest and prospective service to science which remain in manuscript for lack of means of publication. What shall be done in this connection depends, of course, upon the general policy which the Trustees of the Institution may adopt in reference to the matter of publication in general. * * *

Astrophysical Investigations.—The principal object of astrophysical research is to ascertain the physical and chemical constitution of the heavenly bodies, and to trace out and explain the successive stages in their evolution from nebulaæ.

One of the most important pieces of work now required in this field is a systematic photographic survey of the nebulæ in both northern and southern heavens. Photography of the moon and planets under particularly favorable instrumental and atmospheric conditions, with the hope of recording details much smaller than those hitherto obtained in photographs, should also be encouraged.

In spectroscopy there is much to be done. The measurement of the radial velocities of the brighter stars is already well provided for, and as soon as possible this work should be extended to fainter objects in far greater numbers. The spectra of long-period variable stars, studied in connection with their light variations, offer an important field for research. It should be possible with suitable apparatus to photograph stellar spectra on the scale of our present photographs of the solar spectrum, permitting the displacement of lines due to pressure and other phenomena to be investigated. In connection with the study of stellar evolution, many new stellar spectroscopic researches are required. The spectra of nebulæ, particularly of the Andromeda and other spiral nebulæ, also should receive more attention.

The Sun, as the only star whose phenomena can be observed individually, should be thoroughly studied with the best modern appliances. The law of radiation, constitution of the absorbing

atmosphere, spectra and thermal radiation of Sun spots, distribution of faculæ and prominences in latitude and longitude, etc., require extended research. An important investigation for which provision should be made relates to the amount, nature, and possible variation of the total solar radiation.

In the study of the brightness of the stars special attention should be directed to the photometry of the fainter stars and the thorough investigation of variable stars of long period. As all absolute magnitudes are determined visually, further experimental researches are required to solve the difficulties of measuring the brightness of stars by means of photographs. The photometry of asteroids also deserves attention.

In Relation to New Instruments.

As we have stated in the first part of this report, we consider that additional assistants and computers are more urgently needed than new instruments; but it is, nevertheless, true that many important advances can be made as soon as certain new instruments are provided. The construction of a new instrument entails the support necessary for its maintenance in active investigation, and this point must naturally receive consideration in connection with any proposed grant.

We do not feel called upon at the present time to enter upon the discussion of the ownership of instruments; this will naturally depend upon the general policy adopted by the Trustees. Buildings for housing the instruments might perhaps be provided by the institution to which they are supplied, but domes, which are so constructed that they can easily be transported from place to place, might remain the property of the Carnegie Institution, if so desired. We have thought it proper, however, to express some opinion as to the general, or ideal, desirability of a few of the more important projects for new instruments that have come to our attention. These are matters which may be worthy of study in the future, even if it should be found that they cannot be regarded as practical propositions at the present time.

Respectfully submitted.

Edward C. Pickering, *Chairman.*
Lewis Boss,
George E. Hale,
Simon Newcomb,
S. P. Langley,
Committee.

APPENDIX A TO REPORT OF COMMITTEE ON ASTRONOMY.

GENERAL PLAN FOR FURTHERING SPECIAL RESEARCHES IN ASTRONOMY.

In the general report which your Committee has already submitted to the Trustees of the Carnegie Institution it has endeavored to point out some of the more immediate needs of astronomy, and especially such as it has supposed could possibly be provided for with the present means of the Institution.

Your Committee has also considered the possibility of many undertakings for which, on account of the great expense involved, the Carnegie Institution might not be prepared to make provision on a broad foundation at the present time. However, we do not feel justified on this account, in a report which aims to set forth the progress and needs of astronomical research, in omitting mention of these schemes, which may be regarded as parts of a single homogeneous plan. Furthermore, as we shall endeavor to show more fully below, it is quite possible for the Carnegie Institution to make a modest beginning through the appropriation of comparatively small sums from existing funds.

The need for special undertakings in astronomy, such as are contemplated by us, arises in part from the desire to secure special conditions of atmosphere. If the atmosphere were everywhere in a perfect state of calm, with no differences of temperature except those due to increasing altitude, the telescopic images of celestial objects seen through it would be perfectly steady and distinct. Under the actual conditions of observations as we experience them, the telescopic image is in a state of rapid and incessant vibration. On rare occasions there may be improvement over the ordinary conditions, but even at best the full optical possibilities of telescopes are never realized. Fortunately, it happens that at certain points on the earth's surface the meteorological conditions are such as greatly to decrease these difficulties of seeing. Experience has shown that excellent definition is the rule rather than the exception at certain tropical or semi-tropical stations of small elevation above the sea. Indeed, a lofty mountain peak is often inferior from this point of view, though for some purposes it may offer very great, if not indispensable, advantages in that class of observations where transparency to light and heat is quite as essential as good defini-

tion. For example, the latter requirement applies with particular force to researches upon solar radiation.

So far as we know, no single point on the earth's surface unites all the advantages required in the various classes of astronomical observation. It is very probable that a carefully planned search would result in the discovery of sites which would fulfil the requirements of certain special researches more perfectly than any now known, but in any comprehensive plan of research several stations might have to be selected, each adapted to the particular purpose required

In studying these matters from time to time it has appeared to us with increasing force that the similarity of ideas which naturally suggest themselves in the consideration of each topic points to the advisability of treating them together under a single organization.

The object of this organization would be to provide, under the direction of the Trustees of the Carnegie Institution, for the accomplishment of a variety of large and important researches which require special conditions of atmosphere, latitude, or instrumental equipment not now available, by investigators whose previous work shows them to be the best qualified. More specifically, this organization could accomplish three results of the first importance:

(1) Observation by precise modern methods upon objects in the far southern sky which have been wholly neglected hitherto, or insufficiently observed, in order to complete the evidence necessary for generalizations in certain lines of astronomical research.

(2) The utilization of exceptional atmospheric conditions which exist at certain points on the earth's surface, and particularly at great altitudes, for the prosecution of important investigations which can not be undertaken to advantage in the absence of such conditions.

(3) The employment in such researches of the ablest astronomers of all nationalities through the provision of necessary equipment and other facilities required for the special work which they may be prepared to undertake.

Observations in the Southern Hemisphere.

In our general report we have pointed out the need which exists of providing for special observations in the southern hemisphere upon objects which can not be reached by observatories in northern latitudes, with a view to the completion of the evidence which is absolutely necessary for the proper and effective discussion of cer-

tain general problems in astronomy. In a later portion of this Appendix we have treated this subject more fully, and we would respectfully invite the attention of the Trustees to the considerations and recommendations which we have presented under this head.

AN OBSERVING STATION FOR SOLAR RESEARCH.

In a communication which Professor Langley, in response to a request from the Secretary of the Institution, has addressed to the Committee and which we have incorporated in this Appendix, at the end, he has drawn attention to the need which now exists for research upon solar radiation and its possible variations. We are in accord with Professor Langley in believing that a great advance in our knowledge of the physics of the Sun may be anticipated from a research of at least eleven years' duration with special and powerful apparatus, located at mountain stations, presumably in a subtropical region, substantially as he has presented the subject. He estimates that the entire cost of the undertaking would be about $500,000, and the detailed estimates which he has presented seem to warrant that view. The Sun, as the source of those energies which govern the kinetic and vital conditions of the several planets, possesses an unusual interest for mankind. As the only star whose physical condition can be investigated in detail, it offers almost the only opportunity we have for finding the key to the interpretation of problems concerning the evolution and present condition of stars presented through the evidence of the spectroscope.

As will be seen, Professor Langley, in his communication, is impressed with the utilitarian advantage which might possibly grow out of this research. He thinks it possible that by means of this study we may hereafter be able to predict climatic changes of great importance to the welfare of mankind.

We think that the scheme outlined by Professor Langley is worthy of close attention and detailed examination on the part of the Institution.

DEVELOPMENT OF A LARGE REFLECTOR.

The remarkable development of the reflecting telescope in recent years opens up a large field of possible accomplishment through the use of powerful reflectors. The photographs of nebulæ which were made with the Crossley three-foot reflector at the Lick Observatory

by the late Professor Keeler constituted a remarkable advance upon all previous accomplishment in this line. This advance has been still further signalized (according to the hearty, concurrent testimony of a very large number of specialists and leading astronomers) through the recent wonderful photographs of the nebula surrounding the new star in Perseus, and of other nebulæ, obtained by the aid of a two-foot reflector constructed and manipulated by Mr. Ritchey, of the Yerkes Observatory, under the superintendence of Professor Hale. In the latter case there is no doubt that the remarkable results achieved were due not only to the excellence of the two-foot mirror itself, but also in no slight degree to the perfection of the mounting devised at the Yerkes Observatory. Indeed, it appears probable that a five-foot reflector could be constructed which would permit of the examination of the spectra of the brightest stars under a dispersion nearly or quite equal to that which is ordinarily employed in spectroscopic researches upon the Sun, and under conditions as to the use of the spectroscope itself more favorable than has ever before been enjoyed in this class of researches. We believe that if a large reflector constructed on the general plan devised at the Yerkes Observatory could be used at a carefully selected station at some elevated point in southern California, or at some place offering equal advantages in atmospheric conditions, it would be possible to surpass all which has been accomplished hitherto in the photography of nebulæ, in the measurement of motions in the line of sight of the fainter stars, in the precise spectroscopy of typical stars, and in the measurement of heat emanating from the stars. All of the researches would be of the most vital consequence to the progress of astronomy in the lines in which the astrophysical branch of that science is now tending; and should the means become available, we believe that the prospect of success in the direction indicated would be well worthy of the attempt. In a general way we estimate that the installation of such a plant might cost from $100,000 to $150,000, and that it could be maintained in efficient operation by the expenditure of from $10,000 to $20,000 per year, according to extent of operations.

Need for Larger Resources in Astronomy.

We have presented these three illustrations of attempts which it is ideally desirable to make in an effort to secure, in each instance, a unique and remarkable advance in astronomical research. They

are such as readily occur to us in a brief and summary consideration of the present needs of astronomy. No doubt other illustrations of equal importance could be presented by a more careful study of the various problems now presented for solution, and others will certainly present themselves from time to time in the future. It will be seen that in each of the three propositions we have presented it is designed to occupy fields of activity now practically vacant, in answer to an existing demand. In all cases we think the choice of new activities should be limited in this way—always as a response to some need which has naturally and demonstrably arisen out of the development of astronomical progress.

MORE CAREFUL STUDY OF THE NEEDS OF ASTRONOMY REQUIRED.

We have presented these suggestions of plans for the extension of astronomical progress as desirable in the abstract, or ideal, point of view, and we realize that before they could be entered upon as practical propositions, in relation to the means which would be required for their actual exploitation, it would be necessary that they should be examined in much greater detail as to methods of procedure and probable cost than has been possible to us in the brief period which has intervened since our appointment as advisers. Furthermore, we do not consider that the great labor of such detailed examination would be warranted unless the Trustees of the Institution shall have signified their desire to have a more specific presentation of the various subjects. Should it seem desirable on the part of the Trustees that the larger needs of astronomy (of a nature such as we have suggested) should receive more comprehensive investigation and accurate presentation, we are of the opinion that a special commission should be duly authorized to carry out this wish.

If such a commission should be appointed it would be desirable that a sum not exceeding $10,000 should be appropriated to enable the commission to conduct such inquiries as they may deem necessary, including the examination of sites in the southern hemisphere and at great altitudes, the preparation of preliminary sketch plans and specifications for buildings and instruments, etc.

Before leaving the general consideration of this subject we wish to emphasize again the idea which we have stated in the outset, that such an inquiry, if undertaken, should be strictly limited to that class of astronomical investigations which demand special con-

ditions of atmosphere, elevation, latitude, or instrumental equipment. We believe that under no circumstances should there be any interference with present activities. For all work not requiring new and special conditions we think that progress can be most advantageously realized in coöperation with existing institutions, according to the policy already foreshadowed in the preliminaries of the organization of the Institution. Furthermore, we desire to reiterate our view, expressed in the general report and elsewhere, that it is not worth while to give serious attention to plans of investigation for which there is not a good prospect that the services for direction and superintendence of a competent and experienced investigator can be found.

Inasmuch as we have conjectured that it might be possible for the Institution to undertake the establishment of a southern observatory on a modest scale at least, we present our views upon this subject in somewhat greater detail. Following this Professor Langley presents his views regarding an observing station for solar investigations.*

AN OBSERVING STATION IN THE SOUTHERN HEMISPHERE.

Observations Needed in the Southern Hemisphere.—In reviewing the needs of astronomy, those which arise from the insufficiency of astronomical observations which have been made at observatories south of the equator are found to be very striking indeed.

This deficiency on the part of the southern hemisphere extends to nearly all classes of observations of precision. It is especially marked as to the more precise researches in astrophysics.

Owing to the prospective absorption for many years of many of the leading southern observatories in carrying on the astrophotographic survey, there is little prospect that the disparity of southern observations will soon find a remedy on the part of these observatories.

Meanwhile many lines of investigations must seriously suffer for the want of needed observations upon objects in the far southern sky.

We proceed to enumerate some of the most pressing needs for work in the southern hemisphere at the present time:

(1) A recent investigation has shown that the total value of all meridian observations made in the one-fourth of the sky nearest the

* These views are in the accompanying sub-appendix on a Solar Observatory, page 104.

southern pole is only one-fifth that which pertains to the one-fourth of the sky nearest the northern pole. For the faint stars the disparity is much greater. The observations for the determination of the positions of all stars down to the ninth magnitude have been practically completed down to the thirty-second parallel of south declination. It is not known that there is any immediate prospect of a further extension southward of these much-needed observations upon the programme of the German Astronomical Society, and it is difficult to see how any considerable part of the energies of existing southern observatories can be spared for this purpose. It will even be difficult for them to make the necessary meridian observations to provide proper deductions from the catalogue plates of the astrophotographic survey.

(2) The modern revival of interest in double-star measures has extended but feebly to the southern sky. There are no very large telescopes in the southern observatories. By far the largest is the eighteen-inch visual telescope paired with the photographic telescope of the McClean Equatorial at the Cape. Even this is not available for very much work upon double stars.

(3) Much work is being accomplished by several astronomers in the British colonies and at the Arequipa station of the Harvard Observatory upon variable stars. Much work is required, especially in that class of observations requiring telescopes larger than eight inches of aperture.

(4) There is increasing activity in the measurement of stellar parallaxes at northern observatories. Nothing in this line is now attempted upon the southern stars, and very little has ever been done there, except that under the direction of Gill with the Cape heliometer.

(5) The first measurements of the motions of southern stars in the line of sight are yet to be made by the Mills Expedition, which is soon to be sent to the southern hemisphere by the Lick Observatory; but even then we shall have two telescopes there pitted against six in the northern hemisphere. It is very important that another large reflector should be placed in the southern hemisphere and employed for several years, at least, in the measurement of motions in the line of sight.

(6) It has recently been pointed out that the proper investigation of the variation of latitude requires that there should be one or more observing stations in the southern hemisphere. This is a very real and immediate need.

(7) A systematic photographic survey of the nebulæ with a large reflecting telescope is needed at the present time. Photographs should be taken of all important nebulæ in the northern and southern heavens, for comparison with similar photographs to be taken in the future.

(8) Investigations should be made with moderate and high dispersions on the spectra of southern stars in connection with the problem of stellar evolution. A large reflecting telescope would be needed for this work.

In relation to the lines of investigation enumerated in the foregoing, it may be said that all have interest in and for themselves; but in relation to (1), (4), (5), (6), and (8), it may also be asserted that they have a further and very great interest, because the full value of work already accomplished at northern observatories in those lines will not be realized until the corresponding southern observations have been made.

In addition to the lines of investigation we have specified, there are many others which would be of a high degree of interest in relation to a southern observatory. In fact, there are very few activities in northern observatories—saving only experimental researches for the sake of opening new lines of investigation—which do not need to find their counterpart in southern observatories to an extent very much greater than is now possible for existing observatories in that quarter of the world.

Site.—Such an observatory should be south of the thirtieth parallel of south latitude, if possible, for the following reasons:

(*a*) Because the elevation of the southern pole in all the researches specified in the first part of this communication ought to be at least thirty degrees above the horizon in order to secure the necessary steadiness of atmosphere in measurement upon objects near that pole.

(*b*) Because the proper combination of meridian observations with those of the northern hemisphere is best effected when the latitude of the southern observatory is as nearly as possible that of the northern.

(*c*) Meridian observations in right ascension as well as in declination require that the "seeing" shall be good for at least ten degrees below the pole.

(*a*) For an observatory which is to require prolonged and vigorous work of measurement, extreme tropical conditions, such as might seriously affect the health of observers, should be avoided.

It might not be possible to accomplish all the objects desired by a single station, but in such case it would still be desirable to have one observatory as headquarters, and other stations in the most convenient possible relations with it.

In selecting a site, clear skies, dry and equable climate, and a fair degree of elevation above the sea level would obviously be desirable. yet accessibility, stability of local government, and cost of living might modify these considerations to some extent. It would be an advantage, perhaps, to have a site where two intervisible stations, of which one would be at a high altitude, could be maintained. This would be possible in Chile, where the climate is very favorable down to about 35° of south latitude, but where the danger to the fine adjustment of large instruments from earthquakes might be considerable. There would be some advantage in the selection of a point in the interior near Sydney, Australia, where the sky is sufficiently clear, the atmosphere dry, and the climate fairly suited to European and American constitutions. If solar observations should be undertaken at the proposed observatory, there would be a unique and obvious advantage for these, as for many other classes of observations, in having an observatory in a longitude differing so much from that of any existing observatory of importance. Many observations of planetary and other phenomena could be made when it is daytime for all the principal northern observatories. One distinct disadvantage of any station in southeastern Australia is found in the hot waves which prevail in the early months of the year.

South Africa offers a favorable site for the proposed observatory. The civil conditions there may be regarded as somewhat unsettled. The chief observatory of the southern hemisphere is already located there, and it might be regarded as somewhat undesirable that another strong observatory should be located near it.

Organization.—The kind of observatory which we would recommend to be established by the Carnegie Institution must necessarily depend upon the amount which would be available for its establishment and support. In our ignorance upon this point we are unable to present a definite plan, and we simply venture to offer a few suggestions.

Sums varying from $50,000 to $500,000 for plant could be judiciously expended for this observatory. Excellent results might be obtained with an annual maintenance of $20,000; while a much larger amount could be economically expended annually without the least fear that the share of this observatory would be overdone.

In the organization of the proposed observatory the matter of permanent staff would seem to be a secondary consideration. Whatever permanent staff might be maintained should be regarded as auxiliary to the main purpose—special researches. This staff should be held as disposable, on occasion, for the assistance of special investigators to whatever extent the necessity of the case might require. The installation of piers, observing rooms, and even of instruments, could be provided in advance for the use of special investigators by the small permanent staff.

A certain amount of regular observations could be assigned to the permanent staff, such as observations of double stars, variable stars, comets and small planets, when far south of the equator, daily photographs of the Sun, and other observations. For a long time to come it would probably be found desirable to maintain regular meridian observations of southern stars. One astronomer in charge, one or two astronomers of the grade of assistant, and two or three computers would probably suffice for the permanent staff at first.

The permanent equipment at first might consist of a visual telescope of from 12 to 18 inches aperture, a photographic doublet of from 10 to 12 inches aperture, a meridian circle, a zenith telescope, small telescopes, clocks, chronographs, etc.

The equipment for special researches ought to be provided in connection with the special occasion for it, and might, in some instances, remain as part of the permanent equipment at the expiration of the initial investigations which may have called it into existence.

The buildings should be of the most simple and inexpensive construction. The observing rooms should be built as needed, and should be of the lightest construction consistent with the protection and safety of the instruments they shelter.

There should be a simple office building, so planned as to admit of future extension to good advantage. It would probably be a good plan to provide inexpensive living quarters or barracks for observers.

It is to be hoped that the success of the observatory in inviting support would be such that a succession of special investigators, with their respective assistants, could be kept on the ground all the time; so that eventually the distinction between permanent and temporary staff would virtually disappear.

The end to be kept steadily in view should be not to devise employment for a large permanent staff, but to find men of special qualifications to be sent to the observatory from time to time to

accomplish work urgently needed for the progress of astronomy, for which provision can not equally well be made elsewhere.

This subject is so important and presents so many points of novelty that it would be impossible for your Committee during its brief remaining tenure of office to give it consideration adequate for the presentation even of a preliminary working plan. We have contented ourselves, therefore, with the foregoing general suggestions, and we would urge upon the Trustees of the Carnegie Institution the importance of a full and careful investigation by means of a commission specially appointed for that purpose.

EDWARD C. PICKERING, *Chairman*,
S. P. LANGLEY,
SIMON NEWCOMB,
GEORGE E. HALE,
LEWIS BOSS,
Committee.

OCTOBER 21, 1902.

SUB-APPENDIX OF APPENDIX A TO REPORT OF COMMITTEE ON ASTRONOMY.

PROPOSAL FOR A DISTINCTLY SOLAR OBSERVATORY.

OCTOBER 20, 1902.

Professor GEORGE E. HALE:

I have been asked by you for information in some detail regarding the scheme of observations, installation of buildings, and cost of equipment of a solar subtropical observatory for studying the Solar Constant and allied problems at a great altitude, which was proposed in answer to a letter from Mr. Walcott.

I have just returned from Europe and am unprepared to give any exact estimate, but will in the little time at my disposal indicate in general terms what is needed. I have for this purpose taken a list of apparatus actually in use at the Astrophysical Observatory and extended by Mr. Charles G. Abbot, the aid acting in charge, to what might be desired in the proposed installation, as a basis for my estimate of cost.

In my mind the essential idea for the solar observatory is to have two adjacent solar installations in immediate sight of each other; one at a certain considerable altitude, the other at the very highest altitude at which an observer can work, perhaps even only with

special provisions for breathing, for the indispensable condition in future successful study of the heat radiation of the Sun is an altitude where the greater portion of our atmosphere lies below us. This is for the upper site only, which is in connection with the near lower observatory in view from the upper, where the routine work will be carried on.

I wish, while repeating that this altitude is indispensable for some of the objects of the proposed solar observatory, to observe that it is not so for all. I wish also to recall that the whole plan involves primarily the use of special apparatus for solar observations. This primary use admits, nevertheless, that much of this apparatus can be advantageously employed for the study of such other objects as the photography of nebulæ, the Moon, or visual or photographic topography of Mars, though you will notice that these latter purposes may demand special apparatus other than that here indicated, which concerns the Sun primarily.

A necessary preliminary for the choice of any such twin site will be an examination by an expert of the conditions of "seeing" in the daytime for solar purposes only, to find whether the seeing is good for *this* purpose, quite irrespective of the condition of the vision at night, and this should be done soon, if at all, since the time of the next sun-spot maximum draws near.

The researches of which I particularly treat here chiefly involve measures of the *heating* effects of radiation.

1. Principal Objects of Inquiry of a Distinctly Solar Observatory.

(*a*) To determine the "solar constant," so called—that is, the heat equivalent of the solar rays falling perpendicularly upon a given area outside of the earth's atmosphere in a second of time.

(*b*) Whether this quantity be fixed or variable, and if the latter, how it varies through a term of years, and especially what connection exists between such variation and the sun-spot cycle.

(*c*) To determine what absorption the solar beam experiences in passing through the earth's atmosphere. The complete answer to this question implies a knowledge of the transparency of all layers of the air—high, medium, and low—and for all wave-lengths. It should also imply a repetition at both stations of the detailed infra-red line spectrum research already made.

(*d*) What absorption does the solar beam experience in passing

through what may be termed the Sun's atmosphere, and is this absorption constant, or is it, as has been suggested, variable and the cause of the supposed variability of the "solar constant"?

(*e*) What differences are there in the radiation of different portions of the Sun's disc, such as spots, faculæ, and prominences, and what evidence may be afforded from this as to the nature of those phenomena?

(*f*) Another large class of inquiry is not directly connected with heat radiation, but involves the use of the spectroscope, photography, and probably electrical apparatus.

2. Plan of Observations.

Nature of the Work.—Nearly all these researches require a study of the intensity of radiation of selected solar rays of all wave-lengths. The most suitable method for this involves several steps. The first is to obtain by the aid of one or more plane reflectors and appropriate mechanism a fixed horizontal beam. Second, a large solar image must be formed, preferably by a single concave reflector of great focal length. Third, the desired region of the solar disc is selected from this image by receiving it upon a screen with a small aperture, which serves also as the slit of a spectroscopic train. Fourth, a large fixed arm-prism spectroscope, with concave reflectors for collimator and objective, forms the spectrum of the selected solar beam. Fifth, this spectrum falls upon a highly sensitive temperature-measuring instrument and is caused to march uniformly over the sensitive surface, wave-length after wave-length, at a rate fixed by an accurate clock. Sixth, the indications of this heat measurer, exhibiting themselves as to-and-fro rotations of a suspended mirror, are caused automatically to record a curve upon the photographic plate, itself driven at a uniform rate before the mirror by clock-work.

Applications of such Spectrum Energy Work to Particular Objects.—I do not purpose here to give a minute scheme of observations or a detailed statement of the apparatus and accessories which they will require, but merely to indicate the main features. A more minute description, both of procedure and of apparatus, has been drawn up and will be submitted later if desired.

The Atmospheric Absorption.—It will be apparent that question (*c*) (of page 105), relating to the amount and variability of terrestrial atmospheric absorption, must be solved before or at the same time

with the questions (*a*) and (*b*), relating to the amount and supposed variability of the "solar constant," for it is impossible to know how much of this total is lost before the measuring instrument is reached. Experiments near sea-level can never determine this correction with entire certainty, though made with the widely differing thicknesses of intervening air corresponding to different altitudes of the sun, for the atmosphere is almost unbelievably variable in its absorption from day to day and month to month, and even in different parts of an apparently clear day. Especially is this the case with its lower layers, and, besides this, no two layers of air at different altitudes are alike in their absorption. What is required is to get high up in clear air, so that the absorption will be much smaller (it amounts at sea-level to something like 50 per cent of the total radiation), and where that which is left is more constant in amount. Furthermore, dependence ought not to be placed exclusively on observations taken at the same elevation above sea-level, for, while the higher layers of air are less variable in absorption than the lower, and while the absolute amount of absorption is less the higher the observer, yet it is necessary also to take into account the variation in quality of the absorption with the elevation. A second lower station ought, therefore, to be occupied and simultaneous observations made at the high and the low station. This ought not to lead to dispensing with a lower observing station equipped equally as well for work as the higher, and where, indeed, it might prove feasible, *after the study of the air had progressed satisfactorily*, to do much of the observation on the "solar constant." It is clear that the extreme difficulty of observation upon the high station ought thus to be avoided just as far as possible.

Methods of Determining Absorption of the Air and the Value of the Solar Constant.—These problems are naturally to be studied simultaneously. In this general statement I will not describe the procedure special to each, but only indicate several kinds of data that ought to be used. First, solar energy curves taken on the same day at the high station through different air masses to be computed from the altitude of the Sun and the height of the barometer at the several times of observation; second, solar energy curves taken at the low station through similar differences of air mass; third, similar curves taken with the solar beam reflected on an approximate level between distant mirrors both at high and at low stations; fourth, actinometer observations coincident with these several kinds of bolographic work;

fifth, bolometric observations on the spectral energy distribution of the standard radiator or "black body."

The Absorption of the Sun's Atmosphere.—While there is not as good an opportunity to study the Sun's absorption as the Earth's, much may be learned by forming spectrum energy curves at various parts of the disc, for it is apparent that as the limb is approached greater and greater thicknesses of the outer solar layers must be traversed by the radiations from the interior. Thus a comparison of the ordinates of the spectrum energy curves of a solar beam starting at 98 per cent of a radius from the center with that of the central beam itself discloses a powerful absorption of the shorter wave-lengths in this lengthened path through the solar envelope amounting even in the yellow to more than 50 per cent of the whole radiation at the center. It has been supposed by some that there is considerable variation in the temperature and consequent absorption capacity of the solar envelope, and it is to this that Halm, in a recent paper, attributes the eleven-year sun-spot period. The investigation of this question through a term of years by the aid of spectrum energy curves from various parts of the Sun's disc would be of great interest. A comparison of such observations at the high and low station would prove whether or not such results, uninfluenced by the absorption of the Earth's atmosphere itself, may in future be obtained at low stations.

It is unnecessary, after what has been said, to describe at length the particular application of the general method to a study of sun-spots, faculæ, and other solar features, as the suitability of it for this study is apparent.

3. Apparatus and Accessories Required.

The Coelostat.—The first essential for each station is the provision of a fixed horizontal beam of sunlight. While there is opportunity for difference of opinion as to the best provision for this purpose, I am disposed to recommend, on the score of simplicity and satisfactoriness of operation, the coelostat, so called, in preference to any siderostat. I would propose for each station a long polar axis driven at the rate of one revolution in 48 hours, by powerful clock-work, and capable of carrying in the plane of its center several plane mirrors. One of these mirrors, of not less than one meter aperture, is to be used for bolometric purposes. The others may be used to furnish fixed beams for other researches at the same time.

In order to avoid the shifting of the beam with varying declination of the Sun, each beam may encounter near the coelostat a second mirror itself capable of traveling north and south on a track, and to be moved every few days north or south as occasion requires. In order to use the principal coelostat mirror at favorable angles both forenoon and afternoon, its second mirror has two such tracks close to the coelostat, which connect by a curve around the instrument, so that the second mirror may be wheeled to the east or west track, according to the hour angle of the Sun. The other beams for other researches should have only one track position for their second mirrors, choosing the position most favorable for morning or for afternoon observation, as experience would dictate.

While the beams from the second mirrors could be reflected in any direction, a north-and-south one is to be chosen to insure entire invariability of position.

Accordingly the concave mirror of 30 inches or more aperture and of 200 feet focus would be located north or south* of the coelostat some 60 feet, and would send its beam horizontally through a tube *under* the coelostat, where the solar image is received at the slit of the spectro-bolometer apparatus.

I have prepared a further list of apparatus for solar work, with the prices of each piece, but in view of the brief time at my disposal to prepare this preliminary statement, I think it better to give here only the main points.

All this is for the study of solar radiation, principally but not exclusively for that of heat, and it is distinguished by the abundant use of large mirrors, plane and concave, usually in connection with the coelostat and catoptric telescopes. These are associated with special buildings maintaining the bolometric and galvanometric trains at constant temperature, and at the upper station there may be a special construction to enable the observer to work in a special atmosphere. There will also be a special teleferage system or other provision for ready transport to the upper station. There will be two dioptric telescopes of at least 12 inches aperture for photography, and adjuncts too numerous to be mentioned here, but which are indicated fully in a separate note, to be communicated later.

This list of apparatus can later, if desired, be furnished in detail, with estimates for cost of each piece.

The total estimated expenditure for this apparatus, with the buildings, is a little under $150,000; but this does not include the roads

* According to which hemisphere is selected for the observatory.

and other items, whose cost is so uncertain that I prefer to estimate the total expenditure at $200,000 for all such objects.

There has been prepared a scheme of expense for maintenance, including salaries of a director, at $5,000; an assistant director, at $3,000; two assistants, at $2,000 each, with mechanicians and others specifically named, which, with subsistence (necessarily to be provided by the observatory), materials, etc., aggregates $35,000 a year.

It is estimated that $300,000 and interest will provide for this for eleven years, and that the total expense of the distinctly solar observatory for the eleven-year period will be covered by the already named sum of $500,000.

The provision of a great southern subtropical observatory, if associated with this, will doubtless involve a far larger expenditure, which is not here immediately considered.

What here is immediately considered is the idea of a distinctly solar observatory, and even this not solely for its scientific interest, but for its immense possible utilities to the whole human race.

I may say, in illustration, that I am personally deeply interested in the study of nebulæ. I can not but see, however, the enormous difference in quality between this study and that of the Sun, for all the nebulæ in the sky might be blotted out without affecting the price of a laborer's dinner or the material comfort of a single human being. What shall we say of a similar contingency to the Sun? While a slight variation in the radiation of the Sun may conceivably cause the death of millions of men by famine, it certainly seems worth while to look at it from its utilitarian as well as from its purely scientific interest.

It is the possible immense *utility* of the solar observatory that I dwell upon, and concerning which I may borrow the weighty words of Professor Newcomb in a similar connection, and state that astronomical research in this direction may bring to light not merely interesting cosmical processes but "cosmical processes pregnant with the destiny of our race."

S. P. LANGLEY.

[*S. P. Langley to Mr. Walcott, February 28, 1902.*]

FEBRUARY 28, 1902.

DEAR MR. WALCOTT:

You were saying to me that you knew of some persons who might be desirous of aiding, through the Smithsonian Institution, some

large object, and I was led to write you what is in substance the following letter:

I learn from yours of February 14 that you would like to call it to the attention of the Executive Committee of the Carnegie Institution, and, as I have written, I shall be very glad to have you do so, asking you to make it clear that it is in no way a request from the Smithsonian Institution, but a suggestion from me of a great object which Mr. Carnegie himself may care to take up.

I do so the more readily because, considering the Institution wholly apart from its own needs, it would be the glad means of indicating to those who wish some worthy aim for expenditure, some specific object, which may be undertaken if desired *in their own name* and through any worthy medium they prefer.

One of these is the determination of the heat the Sun sends the Earth and the causes of its probable variation. The progress of solar physics has been such in the last few years as to make it of interest to every inhabitant of the planet that this progress should be carried further, not only in scientific, but in economic, and in even humanitarian interests.

The establishment of a great observatory in the tropical or subtropical regions at a high altitude would advance our knowledge of the heavenly bodies in a degree more than could be done by all the physical observatories in the world united. To the founder of such an observatory there would be enduring fame, but it is an affair of a very great deal of money, possibly to be reckoned only in millions. The establishment and maintenance for eleven years of a distinctly solar observatory under these conditions would enable us to study the sun as it has never yet been studied, and through an entire solar cycle, for much less cost.

While this latter research, then, is to be pursued at less cost than the foundation of a great general observatory, it has a specific object of literally world-wide importance and interest.

The determination of the heat the Sun sends the earth annually is the determination of that through which everything on the planet lives and moves, and almost unknown slight variations of this heat are the probable, if remote, cause of the changing character of the seasons and of the lack or plenty in the crops upon the Earth as a whole.

It has seemed possible within the last few years that if we had this knowledge, the years of plenty and of famine could be forecasted as we now forecast a coming storm through the advices of the

Weather Bureau. It is possible, I say, but I do not wish to say more than that it is possible.

I do not know any greater or more worthy object for the expenditure of $500,000 than the settlement of this latter great question would be. It is, with our present knowledge, almost a question of money; but no government is prepared to spend such a sum except for its own interest. This is for the interest of all the people in the whole world, and I entirely concur with the recommendation of its importance from the Chief of the United States Weather Bureau, which I enclose. I should gladly see it undertaken, whoever does it.

Very truly yours,

S. P. LANGLEY,
Secretary.

The Honorable CHARLES D. WALCOTT.

APPENDIX B TO REPORT OF COMMITTEE ON ASTRONOMY.

PROGRESS AND PRESENT STATE OF ASTRONOMY.

BY LEWIS BOSS.

Professor E. C. PICKERING,
Chairman of the Advisory Committee on Astronomy,
The Carnegie Institution.

SIR: Acting upon the suggestion of the Advisory Committee on Astronomy that its individual members submit their views upon various specialties in astronomical research which could not be treated in detail in the general report, I beg leave to submit some memoranda which are suggested by my own experience in watching the progress of astronomy.

As a preface to this, I desire to suggest some views which I regard as of importance in determining the general policy of the Institution toward the support of astronomy.

The United States already has a large number of observatories for which the provision as to maintenance is notoriously small in relation to instrumental plant. There appears, therefore, to be little necessity for the establishment of new observatories or institutions, provided some means can be devised to make better use of those we have.

To construct a satisfactory working policy for the Carnegie Institution in its relation to astronomy, as well as to other sciences, is manifestly to be the work of time and experience. In a general way it might appear reasonable that the Institution should endeavor to accomplish distinct results in definite lines rather than to spread itself over the entire range of astronomy in a miscellaneous way without definite aims. Yet it might be difficult for the Institution to decide upon the directions in which it could most advantageously throw the weight of its support until experience shall have contributed to a solution of the problem. Among the objects it may decide to support at first will be found some that are worthy of continued recognition, both because of their great and obvious importance to the progress of astronomy and because of the efficiency of the particular investigators selected to carry them out.

The massing of miscellaneous astronomical investigations under a single executive head in a great institution does not commend itself to my judgment as economical or as likely to bring to the front the kind of power which is necessary for the highest form of research. In such an institution the main current is apt to be sluggish. It is true that such institutions are valuable when they can be controlled, on special occasions, to the exclusive support of some great investigation like those upon the planetary system which were carried on in different countries at different times by Le Verrier and Newcomb, and that they may also be valuable for purposes of stellar and planetary observation as illustrated at Greenwich, Cape of Good Hope, Paris, and Pulkowa. In general, their usefulness is not in proportion to the expenditure.

Furthermore, we have the expressed wishes of Mr. Carnegie in favor of arriving at the promotion of pure scientific investigation, so far as possible, through aid extended to existing institutions.

Astronomy might be efficiently aided through the maintenance of a new observatory in the southern hemisphere. It is not desirable that this should be an elaborate affair—merely an astronomical station, with a very small permanent staff to serve as a convenience or nucleus for expeditions sent out from the United States for special objects from time to time. Such expeditions have been sent from this country already. In 1850 Captain Gillis, of the Navy, was sent to Santiago de Chile to make observations of the southern stars. The Lick Observatory will shortly send an expedition to measure the motions in the line of sight of southern stars. Dr. Gould's great undertaking for observation of the southern stars, 1871–1884,

was in the nature of an expedition, though it was supported by and became the national observatory of the Argentine Republic. There is now very pressing necessity for such expeditions, as I shall endeavor to show further on.

The foregoing considerations seem to lead to the following suggestions to be commended to the consideration of the trustees as suitable to the present situation so far as it relates to the support of astronomy:

First. Assistance should be extended to institutions which already have in hand important researches in astronomy—institutions which have already demonstrated their usefulness and which have sufficient vitality for a good foundation, but not sufficient income to carry out their existing plans in a desirable way.

Second. To assist investigators of acknowledged ability, wherever found, to initiate or carry out works of high importance to the progress of astronomy; and in doing this to aim at giving the investigator a free hand.

Third. To take into consideration the establishment of an astronomical station in the southern hemisphere for the use of expeditionary forces to be sent out from the United States for the purpose of reducing the great disparity which now exists between observation upon objects in the southern sky, as compared with those in the northern.

As to the investigations in astronomy which are pressing for attention, they will be found to be exceedingly numerous. A few of these I shall attempt to suggest in the sketch which follows, leaving the fields of gravitational astronomy and astrophysics to be covered by other members of the Committee.

PLANETARY OBSERVATION.

The observation for position of the major planets, Sun, and Moon has always been the peculiar care of the Royal Observatory at Greenwich. Such observations have also been made at Paris, Washington, the Cape of Good Hope, and at other national observatories. It is as necessary as ever that these observations should be continued; and the national observatories can be depended upon to meet the demands of science in this respect. As to the observation of minor planets and comets, there is a lack of organized effort. Several observatories in Germany, France, and Italy devote a large share of their attention to these objects. At the Lick Observatory

the discovery and accurate observation of comets has been prosecuted for a long time with much judgment, ability, and success.

Some years ago substantial prizes were offered for the discovery of comets. In those days several enthusiastic amateurs were enlisted in this work, with the result that the sky was constantly patrolled, and astronomers had a reasonable assurance that no comet of any consequence was likely to escape discovery. This is a matter of importance, because it would be interesting to learn whether, as has been suspected, more comets are seen at the times of greatest solar activity, with the consequent inference that the envelopes of these comets are excited by the magnetic condition of the Sun.

METEORS.

The interest in the observation of meteors suffers no diminution. Of late years a systematic effort has been made to photograph some of these objects in flight. These efforts have been attended with partial success. By means of photography, an incalculable advance in the exact astronomy of these bodies becomes possible, and this method is deserving of encouragement. This work has been carried on for several years at the Winchester Observatory of Yale University.

DOUBLE STARS.

Thirty years ago interest in the observation of double stars appeared to be on the wane. The unsatisfactory nature of the orbital computations which were made from time to time on the basis of exceedingly minute quantities of slowly changing relations led to something like a feeling of despair. Comparatively few binary stars have a period of revolution less than one hundred years. Of the vast majority the period of revolution is at present entirely conjectural. Revival of interest in this branch of astronomy was largely due to an American amateur astronomer, Mr. S. W. Burnham, who began his labors with a small telescope. Graduating from this into regular astronomical employment, with the use of large telescopes Mr. Burnham added a large number of discoveries of new and interesting double stars to the list of those already known. He made a very large number of measurements upon these objects. New interest was manifested by other observatories. The large telescopes of the Lick Observatory, of the observatories of Cincinnati, Madison (Wis.), and Washington, of the Northwestern University, and of

the University of Pennsylvania and elsewhere, as well as at various observatories in Europe and the southern hemisphere, have been directed with good effect upon this work; so that there is no reason to complain of a deficiency in this class of observations. The part borne by America in this campaign is extremely creditable and in most refreshing contrast to the history of American astronomy in this respect previous to 1870. In comparison with other branches of astronomical work, the need for increased activity in this line is, perhaps, relatively less pressing.

Researches upon the System of the Stars.

Under this general head may be included a very important part of the work of astronomers of the present day. We may construe it as including meridian and other observations for the position and motion of stars; observations to determine astronomical constants, such as refraction, aberration, and nutation; observations for variation of latitude; determination of parallax, and a great variety of researches which require fundamental observations of precision as the basis. Included in this class of investigations also are meridian observations of planets, upon which our planetary theories are based, since such observations must be made in the same way and in the same series as those of high precision upon the stars.

Observations for Positions of Stars.

There are more than twenty-five observatories in the world wherein observations to determine the positions of stars form the principal activity. There are as many more observatories where such observations are an important part of the work undertaken. At the great national observatories located at Pulkowa, in Russia; Greenwich, in England; Paris, in France, and Cape of Good Hope, in South Africa, in some form or other, such observations are the chief employment, and the fame of those institutions rests chiefly upon what they have accomplished in observations of this class.

We have first exploratory and statistical observations where identification and enumeration are the principal objects to be attained. The great *Durchmusterung* carried out at the Bonn Observatory by Argelander and Schönfeld, the similar survey undertaken at the Cordova Observatory, in the Argentine Republic, and the photographic survey completed at the Cape of Good Hope are the leading

and most effective observations of this class. Nothing of great importance in this line of work has been accomplished in this country. In this same line should be included the great photographic survey now going on under the leadership of the observatory at Paris, which has for its declared object the delineation upon photographic charts of all stars down to the fourteenth magnitude in the entire heavens. There are some indications of lagging in this, the main feature of the coöperative plan described, owing to the great difficulties in execution and the very great estimated cost of reproduction of the plates upon printed charts. This country has taken no share in this great work; and this may be considered a matter of regret. Undertakings of this kind indicate far-seeing insight into the needs of the astronomy of the future, and a spirit of enterprise in the breaking of new paths of investigation. Even the mere counting of stars by Herschel (his "star-gauges") was the foundation of a new philosophy of the heavens—one of the most fruitful chapters in our science. How much greater things may be expected from the charting of the whole heavens upon a uniform plan may easily be imagined. This plan is most interesting from what we know it will accomplish, and not less interesting from the unknown possibilities which may flow from such works.

Fundamental Star Positions.

At the other extreme of the scale is found the determination of the positions of the principal stars—fundamental or absolute observation, as it is called. This problem is twofold. First, we have to find an invariable line of reference, or direction, in space—philosophically one of the most interesting problems which has been attempted in the whole range of exact sciences. The earth is rotating on its axis, with the direction of its axis continually disturbed by a multitude of external attractions and terrestrial perturbations. At the same time, the earth revolves in its orbit about the Sun subject to a multitude of perturbations disturbing uniformity of motion. The earth has a third motion with the Sun, at a high velocity, through space in a direction now approximately known. The problem is to determine with humanly absolute exactness the direction of some line in space in such a manner that it can be identified with certainty at any future time.

The second problem of fundamental astronomy is to ascertain with the highest precision what may be termed the graduation of the sky.

In other words, we strive to determine with reference to some fixed system of imaginary circles upon the sky the location of the brighter stars. This results in the determination of the positions and motions of the stars—the practical outcome. These positions, so determined, become the basis of reference for planetary astronomy, for the tables of the nautical and astronomical almanacs, and for the extension of the work of precision by mere differential processes to the great multitude of fainter stars. Thus, the fundamental observation of the principal stars is the real foundation upon which rests the entire structure of the astronomy of position.

The leadership in extent and importance of work in this line must be accorded to the observatory at Pulkowa and to the two English Royal observatories, respectively at Greenwich and the Cape of Good Hope. Good work of this kind has been accomplished at various other observatories in Europe, but none in the United States up to the present time.

There is, however, a class of observations of precision falling little short of those which are really fundamental, in respect to which the instances of successful accomplishment are more numerous. Some of the notable contributors in this line have been the observatories at Dorpat, in Russia; Königsberg, Berlin, Bonn, and Strassburg, in Germany; Paris, in France; Leiden, in Holland; Oxford, Cambridge, and Edinburgh, in Great Britain; Melbourne, in Australia; Madras, in India; Cordova, in the Argentine Republic, and the observatories of Washington, Mount Hamilton, Cambridge, Cincinnati, Madison (Wis.), and Albany.

After these, upon a scale of precision scarcely inferior in many instances and on a par in importance, rank the observations of the positions of faint stars by secondary methods, either with the meridian circle or through astronomical photography. This is distinct from the general or exploratory charting, which has been described and which is the step naturally preliminary to measurement of precision. The historic zone observations by Lalande, Bessel, Lamont, Argelander, and Gould are examples of this class. To observations of this class we owe a large share of the exact knowledge which has hitherto been acquired as to what is happening in the stellar universe. Some thirty years ago the German Astronomical Society assumed leadership in an undertaking to observe all stars down to the ninth magnitude in the northern sky (over 100,000 stars). The Harvard College Observatory and the Dudley Observatory were participants in this work. The former

observatory and the observatory at Washington have taken part in the extension of this work southward. Lately, at Albany, observations of stars down to the eighth magnitude have been extended to the thirty-seventh parallel of south declination, the results of which are now in preparation to form a catalogue of 10,000 stars.

Included in the program of the great astrophotographic survey under the leadership of the National Observatory at Paris (to which allusion has already been made in the foregoing), is a plan to determine through measurement of photographic plates the positions of all stars brighter than the eleventh magnitude. This gigantic undertaking is proceeding with a success as to the northern hemisphere which scarcely could have been anticipated by men practically acquainted with the difficulty of such tasks. The state of the work in the southern hemisphere is far less flourishing, though the situation is not without an element of hope. The burden of this great enterprise has fallen very largely upon the English and French governments. The United States has taken no part in it.

It may become desirable in the future that the Carnegie Institution should undertake a section of the southern sky, should some one, or more, of the participants in the southern hemisphere fail—a contingency not without the bounds of possibility.

In the foregoing paragraphs I have indicated in a very summary way what has been accomplished in observations of precision upon the relative positions of stars. This work has exercised the energies of considerably more than one-half the working force of astronomers during the nineteenth century. The observatories already established for, and now engaged in, this work may be depended upon to keep up a fair balance of output in relation to other branches of astronomy, so far as the ordinary types of such work is concerned, and in relation to the northern hemisphere. In all countries except the United States, governments may be relied upon to render fairly adequate support to this class of observations, since the relations of this work to the practical uses of mankind, notably in geography and navigation, appeal more directly to the motives of official administration.

Fundamental determinations of star-positions, however, belong to a class of highly specialized researches, giving free play to originality of conception and invention of methods. Only a very few of those which have been made in the entire history of modern astronomy are entitled to the claim of high rank. To sustain the proper balance toward differential, or secondary, observations, there should

have been twice as many. This work is extremely laborious in extent of its details and exacting in the attention it requires to minute and hidden sources of discordance. It has been described as the most severe example of physical investigation in the exact sciences. When the opportunity offers, such researches might appropriately be assisted by the Carnegie Institution to the great profit of astronomical progress.

In fact, there is no lack of novelty and scientific interest in meridian observations for positions of stars when they are directed toward the investigation of special problems which require observations in a special field. In view of the relations of our government to astronomy, it also seems proper that the Carnegie Institution should attempt more in this line than it might under different circumstances.

There is great deficiency of such observations in the southern hemisphere, while for the purposes of astronomy it is important that the southern hemisphere should receive equal attention with the northern. There is especial need of meridian observations in that hemisphere now. Sir David Gill, Astronomer Royal at the Cape of Good Hope, is accomplishing a great amount of meridian observation of a very high grade, and measures lately taken by him seem to insure still greater and better results in the future. But a single observatory is entirely unable to cope with the situation; and, although much good work may be expected from other observatories south of the equator, there is pressing need that further provision for meridian observations of all classes should be made. The location of an observatory for this purpose should be south of the thirtieth parallel. The Harvard Observatory has extended its photometric and photographic patrol to the south pole by means of its station at Arequipa, in Peru. Recently the Lick Observatory has dispatched an expedition to extend its determination of the motion of stars in the line of sight to the brighter stars of the southern skies. That fundamental and secondary meridian observations should be made in supplement to existing forces in that quarter of the world is sufficiently obvious to all who have closely studied the conditions of the problem involved. This could be accomplished under a plan resembling, with modifications, that which was adopted by Dr. Gould thirty years ago. The United States has no territory in latitudes suited to the establishment of such an observatory, and there is small likelihood that its attention would be favorably enlisted in the consideration of such a plan. Such an observatory might be a very simple affair. It might be purely expeditionary in form, and it

might even be supplied with instruments temporarily borrowed from some northern observatory.

In general, that class of investigations in this department of astronomy, which require some degree of departure from existing routine, are very likely to require attention at times through supplementary aid. The whole subject is one well worthy of the attention of the Carnegie Institution.

Deduction from Meridian Observations.

The observer in this department of astronomy corresponds very well with the skilled collector in the various branches of natural history. The observations, which have been accumulated for more than a century, must be collated and compared. They must be brought to bear upon the solution of physical problems. They are dead material until they have been used in this way. Very little has been done with this vast mass of precision observations in the way of bringing its results to bear upon questions concerning the motions of the stars, the motion of the Sun through space, and the structure of the sidereal universe. The answers to a variety of most important questions in cosmogony that are propounded and vaguely discussed lie hidden in that mass of material, answers which very probably may bring us only a few steps forward, but which are certain to accomplish as much as that.

Comparatively little is yet known about the motions of the stars. Not one-tenth of the available observations has been brought to bear on that subject. Scarcely a greater percentage has been utilized in the discussion of the all-important constant of precession, and the same is true as to that problem of most absorbing interest—the solar motion.

Within the last three years the immense work of collecting and comparing this mass of meridian observations has been undertaken by Dr. Ristenpart, under the auspices of the Berlin Academy. Experience will show that this work must be continuous, since it will no sooner be completed to a certain date than the necessity will arise for extending to a later one. Yet it is from work like this that we must derive nearly the whole of the benefit to the progress of astronomy which is to be expected as the ultimate reward for the immense energy which has been expended in making meridian observations of precision.

For many years a similar collation and comparison has been in progress at the Dudley Observatory of Albany, restricted to stars

known to have sensible motions, or to be brighter than the seventh magnitude. This has been undertaken with the purpose of founding upon it a comprehensive examination of problems relating to precession, solar motion, and generally to systematic motions of the stars. In this way only is it possible to gain exact knowledge as to the mechanism of the heavens.

Reduction of Old Observations.

In recent years there has been great activity in the work of revising the computations relating to series of meridian observations made prior to the middle of the last century. This is a most important department of astronomical effort. Several years ago Dr. Auwers, of Berlin, completed a most painstaking and masterly revision of the star catalogue founded on Bradley's observations of mean date about 1755. He also effected a new reduction of Mayer's less important observations of about the same date. Stone and Gill have completed the reduction and publication of the long series of observations made at the Observatory of the Cape of Good Hope. Frisby and Brown, of the Naval Observatory, have reduced the zone observations of Gilliss at Santiago in 1850. Weiss has revised Argelander's southern zones of 1850, and Seeliger those of Lamont of an earlier date. Dr. Copeland, of Edinburgh, has undertaken to revise the reductions for Henderson's valuable series of meridian observations made at Edinburgh around 1840. Dr. Downing, of the British Nautical Almanac, has published a revision of Taylor's Madras observations contained in his star catalogue for 1835. For some years Dr. Hermann Davis, of the U. S. Coast and Geodetic Survey, has been engaged upon a new reduction of Piazzi's observations made at Palermo about 1800. Recently a new reduction of Groombridge's observations, 1810, has been undertaken at the Greenwich Observatory. Dr. Auwers has recently published a most valuable star catalogue, compiled from the Greenwich observations of Pond about 1815, and Dr. Chandler long ago proved the very great value which would attach to a new reduction of Pond's later (and best) work about 1830.

It is extremely desirable that this latter work should be taken up and pushed to completion, since it would undoubtedly result in a star catalogue of very high precision, fairly comparable in accuracy with the very best observations of Struve and Argelander and superior to them in some respects.

This field is important, and much remains to be done. One is tempted to urge that an organized effort be made to revise all the older star catalogues and planetary observations in a manner to render their best results available for the immediate and pressing needs of astronomical investigation. Certainly this work is worthy of aid wherever it is undertaken upon the initiative of competent hands.

DETERMINATION OF STELLAR PARALLAX.

Closely associated with the work of determining the positions and motions of the stars is the problem of determining their distances through measurement of parallax. Sixty years ago Bessel was the first to measure successfully the parallax of a star, after this problem had baffled the most strenuous efforts of many able astronomers from time to time in the past. Since Bessel's measure of the parallax of 61 Cygni other measures have been slowly accumulating, until, within recent years, there has been very greatly increased activity in this line of researches. Parallaxes are measured by many different methods, of which those most approved at present are by means of the heliometer, by means of photography, and by differential transits on the meridian. Gill at the Cape of Good Hope and Elkin at New Haven have determined a large number of parallaxes by measurements with a heliometer. Recently Elkin and Chase have been carrying on a kind of parallactic survey upon a very large number of stars. The photographic method has been tried with some degree of success by Pritchard of Oxford, and by others. The transit method was first illustrated by Kapteyn, at Leiden, and more recently by Flint at Madison (Wis.). Investigators are beginning to realize that the measurement, one by one, of the parallaxes of individual stars is a task which can no longer be postponed. There is every reason to believe that the photographic method, when applied with the aid of a large telescope of special design, like that proposed at the Yerkes Observatory, may yield results economically and, in all probability, of a high degree of accuracy.

VARIATION OF LATITUDE.

Researches upon this subject have been of great interest ever since Dr. Chandler discovered, a few years ago, that the pole of the earth's figure rotates about the pole of rotation. The consequences of this phenomenon are so interwoven with the determination of the

places of the heavenly bodies and of the great constants of astronomy as to induce an organized effort to investigate its character in the most complete and precise way. A large number of series of observations has been made during the last seven or eight years with the special object of determining the actual variations of latitude in various quarters of the world. Recently, upon the initiative of the German Geodetic Association, international observing stations, suitably distributed upon the same parallel of latitude, have been occupied by astronomers. Such a station has been established at Gaithersburg, Md., and at Ukiah, California; and another is provided by the Observatory at Cincinnati. It has been strongly urged that similar stations should be established in the southern hemisphere. This is a subject which is well worthy the attention of our government, and should be urgently pressed for its favorable consideration. It is quite possible that in the future this subject in some of its phases may deserve serious consideration by the Carnegie Institution.

Astronomical Constants.

None of the great constants of astronomy is yet determined with the accuracy which modern science demands. In recent years the solar parallax has demanded an amount of attention which seemed at times disproportionate to that which was devoted to other important fields of astronomical effort. The support which was extended by governments to expeditions for the observation of transits of Venus in 1874 and 1882 was munificent beyond precedent, and probably beyond the actual requirements of the case. Our government was even more liberal than any other. It is not known whether the observations by parties sent out from the United States were ever completely reduced. Twenty years after the last transit find them still unpublished.

Meanwhile other methods for the determination of the solar parallax have been applied, apparently with a result worthy of greater confidence than that due to observations of transits. Nearly a quarter of a century ago Gill employed the heliometer with good result in observations upon Mars at Ascension Island. Later, Gill and others employed the heliometer in coöperating observations upon minor planets favorably situated for the determination of parallax. The constant which is generally adopted at present virtually rests upon these observations. A few years ago a new planet, Eros, was discovered, remarkable for its eccentricity and its occasional

near approach to the earth. In 1900 occurred an opposition of this planet which was more than ordinarily favorable for the determination of parallax. A very elaborate scheme of observations by observatories in coöperation was organized under the auspices of the Paris Observatory. An immense mass of observations was accumulated, especially of photographs. There seems to be a suspicion that the computers are somewhat paralyzed by the very abundance of material which exists in these photographic plates, contributed from all parts of the world. At the Lick Observatory, as well as at some others in this country, a very large number of plates was secured and is now awaiting measurement and reduction. It appears that little or no progress in this work has been effected as yet, and doubtless the Carnegie Institution may be asked to consider how far and upon what principles it is willing to extend its aid in carrying on this work. One is tempted to observe that zeal in taking photographs and reluctance in making measurements upon them when taken is rather characteristic of this branch of astronomy when applied to special objects. The office of the photograph is to bring down the counterfeit presentment of the sky to the laboratory desk, where measurements can be made at ease. The photograph does not eliminate the necessity for real work. It is simply supposed to make the work of measurement easier. The real contribution to science does not fairly begin until the photographs have been measured, and it does not seem necessarily to follow that because a stack of photographs has been taken with a certain object in view, these must be measured in order to save the results to science.

Aberration of Light.

In recent years interest in the constant of aberration was renewed in a remarkable degree. Astronomers, after resting for nearly a half century apparently satisfied with Struve's historic determination, suddenly suspected that it might be subject to serious revision. For several years there has been very great activity in observations and computations bearing upon this constant. The determination of this constant is interesting, both from its relations to the solar parallax and to the velocity of light. It is extremely desirable, therefore, that this constant should receive the attention astronomers now seem disposed to accord to it, and that variety of method should be encouraged. Recently in this country determinations have been made by Professor Comstock, of Madison, employing the

Loewy method; by Professor Rees at Columbia, and by Professor Doolittle at the University of Pennsylvania, using the zenith telescope, and by Professor Asaph Hall, Jr., at Ann Arbor, from meridian circle observations. The discordance of the results seems to render it desirable that further determinations should be made. Quite recently Dr. Chandler has carried on a series of critical investigations by computation upon all the series of observations available in the discussion of this constant, and he has arrived at conclusions of remarkable interest.

Constant of Precession.

The constant of precession is also of very great fundamental importance in its bearing upon the entire range of astronomy of precision as well as upon the testimony upon which we must ultimately rely for our investigation of the mechanism of the universe. This constant, however, stands in such intimate relation to the solar motion that the discussion of the two is practically inseparable. Special observations for determination of this constant are not necessary.

It should be mentioned that in Professor Newcomb's great work upon the planetary system he devoted much attention to the evaluation of a series of astronomical constants which should be consistent with each other and as precise as possible upon the basis of the material of discussion available to him. These constants have been generally adopted in the construction of astronomical almanacs; and it is characteristic of the rapid progress of astronomy that there has been no time within the memory of living astronomers when there has been apparently less satisfaction in the existing values of those constants which have been adopted. This is not because Professor Newcomb's results are not an improvement upon what had gone before, but because of the rapid accumulation of material of observation which points to the chance of deriving more reliable values, corresponding to the more exacting demands of astronomy.

Related Lines of Research.

From an analysis of the foregoing, it will be seen that there are several related lines of investigation pressing for attention, all bearing upon a distinct problem—that concerning the structure of the sidereal universe.

Of very high importance in discussing the minute motions of the stars is it that the observations of position should be freed from their

systematic errors—errors which, though they are not important in the determination of the motion of one star, are vital when masses of motions are treated to determine the general drift of stars. To arrive as closely as possible to freedom from systematic error, we must multiply fundamental determinations of position.

For the study of the mechanism of the sidereal universe we need to know the motions of the stars upon the basis of all the observations which have been made, correcting these, so far as possible, for their systematic errors. Then we shall be able to arrive at a better determination of the solar motion than has been thought of up to the present time, and we shall develop new ideas as to the drift of stars in various regions of sky.

This research, associated with determination of stellar parallax and with the measurement of motion in the line of sight, makes a strong combination for solving the problems that concern the distribution of the brighter stars in space which is relatively near to the solar system.

Determination of more accurate constants of aberration and precession are so interwoven with this problem that they must be considered an integral part of it. The same, in a modified degree, is true of the measurements of variation of latitude.

Without prejudice to other important lines of astronomical research, these investigations seem to offer a peculiarly favorable opportunity for concerted effort toward a highly important end to be pursued for several years to come. With the aid which the Carnegie Institution might be able to render, this program could be pushed forward with great certainty to a successful stage of development. This programme offers the advantage of a distinct purpose toward which many lines of research converge, each important in itself and all deriving greater importance from their intimate association.

Respectfully submitted.

LEWIS BOSS.

Appendix C to Report of Committee on Astronomy.

PROGRESS AND PRESENT STATE OF CERTAIN DEPARTMENTS OF ASTROPHYSICAL RESEARCH.

By George E. Hale.

Professor E. C. Pickering,
Chairman of Advisory Committee on Astronomy.

Sir: The remarks in this paper are made with special reference to certain researches which deal with the physical and chemical constitution of the heavenly bodies. The science of astrophysics, which makes such researches its principal object, has developed with great rapidity during the half century that has elapsed since its foundations were laid by Kirchhoff and Bunsen. From beginnings which of necessity involved much work of a purely qualitative character, the science has advanced to a position in which the demand for precision is no less exacting and is no less fully attained than in the most rigorous astronomical investigations. The nature of the methods employed and the possibilities of instrumental construction permit us to hope that a still higher degree of precision may be expected in the immediate future.

The general suggestions embodied in the report of the Advisory Committee meet my views at all points. There can be no doubt that the most serious need of funds is for the more complete utilization of existing equipment. One has only to contrast the extensive instrumental outfit of our observatories with the wholly inadequate provision for assistants and computers in order to realize this. Furthermore, there is great need for the establishment of an astronomical institution which can provide facilities, especially in the southern hemisphere, for the prosecution of special researches. The plan for this observatory need not be discussed here, as it is fully set forth in Appendix A. But I wish to offer some remarks on the importance of new instruments, as it is possible that the statement of the Committee on this subject may be misinterpreted by some who are unfamiliar with the nature of astrophysical research. Let me repeat, however, that I regard the needs here suggested as second in importance to the present requirement of more assistants and computers.

The extensive equipment of American observatories has been alluded to in the report. Without important additions of apparatus, twice or three times the present number of workers could find profitable employment for many years in making observations with existing instruments, and reducing them. But this fact by no means proves that our present equipment, powerful though it be, is not open to radical improvements, in which may lie the greatest hope of future progress. The history of science has shown that a single improvement in instruments may render years of work with inferior apparatus unnecessary. This is particularly true of astrophysical research, which, in view of the nature of the phenomena investigated and the possibility of employing the innumerable devices of the physicist, cannot be considered as subject to precisely the same conditions that obtain in researches dealing exclusively with the positions and motions of the heavenly bodies. In such researches the instruments and observational methods are comparatively few in number, and in the course of years a definite system of procedure, departed from only in matters of detail, has been gradually evolved. But astrophysics, which is so closely akin to physics, may be regarded almost in the aspect of an experimental science, susceptible of indefinite growth in method and in scope. No sharp line can be drawn separating its investigations from those of astronomy on the one hand or those of physics on the other. The primary interest of the astrophysicist may be an astronomical one; but it may equally well be that of the physicist, utilizing stellar temperatures and pressures to solve problems of atomic structure for which laboratory appliances are inadequate.

Under these circumstances it will be understood that as long as astrophysics continues to be a growing science the provision of new instruments for its researches will be necessary. A concrete example, drawn from an investigation of the evolution of the red stars, will illustrate the nature of the instrumental needs that constantly arise in the present state of the subject.

The study of stellar evolution is based upon the spectroscopic analysis of starlight. With our largest telescopes it is perfectly possible to photograph the spectra of stars much fainter than those at the limit of naked-eye vision. Measurements of the positions of the lines in these spectra determine the chemical composition of the star's atmosphere, set an upper limit to the pressure in it, and give an accurate value of the star's velocity in the direction of the earth. In most cases such spectra can be arranged in a definite series,

showing the gradual changes from star to star, which mark the steps in their evolution from nebulæ.

Every research bearing on stellar evolution is certain to raise questions of great interest, to answer which additional stellar, solar, or laboratory researches will be required. The spectra of red stars, for example, have been found to resemble in a most interesting way the spectra of Sun spots. On the strength of this one might be tempted to conclude that these stars, which probably represent the last stage of stellar evolution, are covered with spots like those on the Sun. Such a conclusion would have an important bearing on theories of the Sun, and perhaps on theories of variable stars. But with existing instruments the question cannot be settled. What is needed is a powerful grating spectroscope, such as Rowland used in his work on the Sun, mounted immovably in a constant temperature laboratory, and supplied with starlight by a special form of reflecting telescope. The special conditions thus realized would permit the exposure of a photograph to be continued from night to night, thus compensating for the faintness of the highly dispersed starlight. Even with very long exposures, however, only a few of the brightest red stars could be investigated in this way. For this reason the reflecting telescope employed should be of the greatest possible aperture. Many other astrophysical researches, such as the determination of the amount of heat radiated by the stars, could be made with such apparatus.

Thus one of the first questions raised by this study of stellar evolution suggests a new combination of instruments, which should render possible many important advances. Another question illustrates the urgent need for solar research. Available information regarding the spectra of Sun spots is wholly inadequate for the purposes of this investigation of the red stars, or for any systematic study of the spots themselves. It is remarkable that so many applications of the best spectroscopic methods to solar investigations are still to be made. Here, as in the case of the red stars, the nature of the work to be done is such as to require the use of instruments now employed only in the physical laboratory. If a small fraction of the enthusiasm so admirably shown on the occasion of total eclipses with a moderate share of the funds could be devoted to solar research at home, our knowledge of the Sun would lose nothing by the exchange.

But even the simultaneous study of the Sun and stars with instruments much better adapted for the purpose than those now employed

will not suffice in a thorough investigation of stellar evolution. Constant appeal must be made to laboratory experiments for the elucidation of celestial phenomena. The fact that some of the information required may be obtained at a future date in some physical laboratory by no means meets the need for immediate results. Cases are constantly arising in which much time could be saved if a good collection of physical apparatus, especially designed for radiation experiments, were at hand. But it is not only for incidental researches that such apparatus is needed. The advantage of carrying on extended physical investigations in direct connection with solar and stellar work is frequently very great. Evidence of this, based upon the peculiar radiation phenomena of hydrogen in certain stars and of carbon in others, the lack of extensive information regarding the effect of pressure on radiation, etc., might be multiplied indefinitely. The bearing of this on the importance of laboratory work in observatories and incidentally on the nature of astrophysics and its needs is sufficiently obvious.

This illustration of the intimate relationship of solar, stellar, and laboratory phenomena, drawn from a single research on the evolution of the red stars, is intended to give an idea of the variety of the questions which present themselves in astrophysical investigations and the necessity of providing new instruments to answer them. Consideration of such questions as these has led to the following conclusions, which seem likely to have some bearing on the progress of astrophysics.

1. In many investigations it is desirable that stellar, solar, and laboratory researches should be planned and executed from a single point of view, and, if possible, in a single institution. For this reason observatories in which astrophysical work is done should be well equipped with physical apparatus in order that special laboratory researches can be made when necessary.

2. Solar researches in great variety are urgently needed. Few fields of investigation offer such opportunities for important advances.

3. Laboratory conditions should be more completely realized in stellar and solar research. In this connection it is worth while to recall the fact that the spectroscope, even more truly than the telescope, is the chief instrument of the spectroscopist, and hence that provision should be made for its use under the most favorable conditions.

4. Much attention should be devoted to the improvement of photographic plates and processes. A comparatively small advance in this direction may double the power of a telescope.

5. The development of new instruments and methods should be encouraged. The more general provision of instrument shops in connection with observatories is to be recommended for this purpose.

6. The construction of large reflecting telescopes offers the greatest possibility of improving the instrumental means of astrophysical research. For all classes of stellar spectroscopic work, the measurement of the heat radiated by the stars, the photography of nebulæ, and many similar investigations, the reflector has very great advantages over the refractor. It is safe to predict that many future advances will be made with its aid. (See page 141.)

In closing these introductory remarks, I wish to add a word on the question of coöperation. In seeking to further the interests of astronomical and astrophysical research through coöperative effort, it seems to me that great care must be taken not to defeat what is perhaps the highest aim of the Carnegie Institution—the encouragement and development of individual genius. Cases will doubtless arise in which guidance of many workers by some well-known authority may be desirable and even necessary to the accomplishment of far-reaching results; but for the most part such cases will involve only the application of routine methods, sufficient for the collection of a mass of data needed in certain investigations. My argument is directed mainly against the centralization of authority and its concentration in a few individuals, however able they may be. It seems probable that the greatest progress will come rather through the advent of new minds, each competent to select its own point of view and to plan researches unhampered by the regulations of established systems. For this reason I believe that the best returns will be realized from assistance given to individual workers, coöperating, it may well be, with others, but constantly encouraged to advance science through the development of ideas and methods of their own.

The following sections contain statements of the progress and present state of certain departments of astrophysical research, for reference in connection with the report of the Advisory Committee.

Stellar Spectroscopy.

Although Fraunhofer discovered in 1823 that the spectra of stars differ among themselves and from the spectrum of the Sun, it was not until 1859 that spectroscopy was established upon a firm basis by Kirchhoff and Bunsen. Their analysis of sunlight by an instrument which permits the chemical and physical phenomena of distant

luminous objects to be studied with certainty served as a foundation for the recent remarkable development of this subject. Secchi, shortly after the chemical constitution of the Sun had been discovered, placed a prism over the objective of his telescope in the manner instituted by Fraunhofer, and proceeded to survey the spectra of the stars. In 1863 he published his first classification, which showed that white, yellow, and red stars have characteristic spectra, so related as to indicate a general line of development. A short time later Rutherfurd, who had initiated such work in the United States, published a similar classification. In 1864 Huggins entered upon his career as a pioneer in celestial spectroscopy. The record of his discoveries is too extensive for more than the barest reference here. He first directed his attention to stellar spectra, and after making visual observations which established the presence of iron and other elements in the stars, he attempted to record their spectra by photography. His first plates, though they contained impressions of the spectra, did not reveal the lines. These lines were first successfully photographed in 1872 by Dr. Henry Draper, who actively pursued this work until his death in 1882. In 1876, with more powerful instrumental means, Huggins returned to the task with great success. Thanks to the use of suitable refractive and dispersive media, his photographs reach far into the ultra-violet. They revealed at once the characteristic series of lines in the spectrum of hydrogen, previously unknown even in laboratory researches. The publication by Balmer of an empirical formula which accurately represents the lines of this series stimulated the studies of spectral series by Kayser and Runge, Rydberg, and others, which have so greatly increased the range and significance of spectroscopic research.

Two fields of investigation, both successfully cultivated in recent years, were opened up by Huggins. His identifications of spectral lines were significant, not merely in disclosing the chemical composition of stellar atmospheres, but also in suggesting relationships of star to star which could be possible only on the assumption of descent from a similar ancestor. The investigation of stellar evolution, foreshadowed in the classifications of Secchi and Rutherfurd, was thus placed upon a sure footing. Through the researches of Huggins, Vogel, Dunér, Pickering, Lockyer, and others, the line of development from gaseous nebulæ through Sirian and solar stars to the cooling red stars has been traced out with few elements of uncertainty. Where uncertainty still exists, it almost invariably arises from the fact that many of the most interesting and impor-

tant stars are very faint. The future study of stellar evolution, especially in its more detailed aspects, which have as yet received little attention, will, therefore, in large measure be confined to the most powerful telescopes obtainable. In work of this kind every inch of aperture counts, and instruments far larger than any now available could be used to immense advantage.

The other field of stellar spectroscopic research opened up by Huggins relates to the motions of the stars in the line of sight. Early recognizing the astronomical significance of Doppler's principle, Huggins proceeded in 1868 to determine stellar motions with its aid. Viewed from our present standpoint, the attempt was a bold one, for with the instruments at his disposal and in the absence of the almost indispensable aid of photography, the detection of the minute displacements of stellar lines which result from the star's motion with respect to the earth was next to impossible. It is therefore not surprising that Huggins' early results show but little agreement with those obtained by modern methods. The important fact remains that an immense field was opened up through these pioneering efforts.

The prosecution of this research at Greenwich, though persisted in for many years with inadequate facilities by Maunder, yielded few results of value. It was not until Vogel, whose first attempts had been made in 1871, again attacked the problem at Potsdam in 1887 that the great possibilities of the method were in any degree realized. Profiting by the advantages offered by photography, and employing a spectrograph especially designed for this research, Vogel substituted for the fluctuating and unsteady image seen at the telescope, a photographed image of the spectrum, accompanied by a comparison spectrum of hydrogen or iron. The plates, measured under a microscope in the most favorable laboratory conditions, yielded the first reliable determinations of radial velocity. With the exception of a single classical research by Keeler, who in 1890 measured with remarkable precision the radial velocities of the planetary nebulæ and certain stars by visual observations with the Lick telescope, all accurate determinations of this nature have been obtained by the aid of photography.

Once set on foot by Vogel, the study of radial velocities advanced rapidly. In 1898 a great step forward was made by Campbell, whose improvements of methods, embodied in the Mills spectrograph of the Lick Observatory, enabled him to secure results far surpassing in precision those previously obtained. After measuring the radial

velocities of several hundred stars in the northern heavens, he is now proceeding to South America to continue the work in the southern sky.

An important outcome of radial velocity determinations was the discovery of the so-called "spectroscopic binaries," which are double stars whose component members are too close together to be separately distinguished with any telescope. The rapid orbital motion of these bodies is betrayed by oscillations of the spectral lines. When only one of the bodies emits sufficient light to produce a spectrum, the only change observed is a to-and-fro motion of the lines, which are displaced toward the red while the star is moving away from us, toward the violet while the motion in the line of sight is in the direction of the earth. If both components are luminous, the lines of the compound spectrum are periodically doubled through the effects of the orbital motion. Some fifty spectroscopic binaries have already been discovered. So numerous are they that Campbell estimates them to comprise one-seventh of all the stars.

Researches on the radial velocity of stars with powerful instruments are in progress at the Lick, Yerkes, Potsdam, Meudon, Cambridge, Pulkowa, and Cape observatories. At present most of the work is confined to the brighter stars, but it has been found at the Yerkes, Lick, and Potsdam observatories that the velocities of stars as faint as the eighth or ninth magnitude can be measured with fair precision with large telescopes. As the velocities of a great number of stars must be ascertained for use in studies of the structure of the universe, as well as in determinations of the motion of the solar system in space, it is evident that a system of coöperation between observatories engaged in the work should soon be arranged. Hitherto the necessity of developing methods and eliminating sources of systematic error has rendered independent work desirable, though certain standard stars are being systematically observed through mutual agreement of the above-mentioned observatories. Within a short time the period of preparation and experiment will have passed, after which a general plan of coöperation should be adopted.

Two important fields of stellar spectroscopy have yet to be noted. The study of the spectra of variable stars, particularly those of long period, for which important results have already been obtained by Fleming, Bélopolsky, Campbell, and others, promises an abundant harvest of results, if sufficiently powerful telescopes can be used for the purpose. In general, spectroscopic and photometric observations of these stars should be made simultaneously. The other

problem relates to the distribution in the heavens of stars of various spectral types. The extensive photographic surveys of the Harvard Observatory have provided the most abundant material for this study. The method of placing prisms over the telescope objective, employed by Fraunhofer and Secchi in their visual work, is here applied photographically. Thus the spectra of several hundred stars are obtained on a single plate. The entire sky has been photographed in this way. An examination of these plates has led to the discovery of a large number of objects of interest, among them six novæ, seven spectroscopic binaries, 75 stars of the fifth type, nearly 200 variable stars, besides 500 which occur in clusters. A detailed study has also been made of the spectra of the brighter stars north and south, several hundred lines being sometimes discussed in the spectrum of a single star.

Stellar Spectroscopes.—A brief consideration of the steps by which the instruments employed in stellar spectroscopy have been employed should be instructive in its bearing on further progress. The earlier spectroscopes gave small dispersion, and in most cases were merely the ordinary small instruments of the laboratory attached to telescopes. When photography was first applied by Huggins it became apparent that special devices would be required to keep the star on the slit throughout the exposure. Hence his use of polished slit jaws, on which the reflected stellar image could be watched with an auxiliary telescope. The attempt to measure radial velocities made higher dispersion necessary and resulted in Vogel's specially designed spectrograph, in which freedom from flexure was an essential feature. It soon appeared, however, that if the prisms varied appreciably in temperature during the exposure, displacements of the lines would result. Hence Deslandres introduced apparatus for maintaining the prism box at a constant temperature, now universally employed. The success of the Mills spectrograph was due to no radical change in design or method, but rather to the use of a larger, stiffer, and more powerful instrument, improved in both optical and mechanical details, and employed under favorable conditions with a telescope of great light-gathering power. Frost's design for the Bruce spectrograph is a further step in the same direction. Each of the attachments for producing the comparison spectrum recently adopted at Mt. Hamilton and Potsdam embodies a distinct element of advance. In the new Chile spectrograph Campbell has introduced a radical modification of the mechanical design, for the purpose of reducing flexure. The optical parts

of this instrument differ in no important respect from those of the Mills spectrograph. The complete realization of laboratory conditions in stellar spectroscopy is the essential purpose of the experiments now being pursued at the Yerkes Observatory. A large grating spectrograph, rigidly mounted in a constant-temperature laboratory, will receive the star's light from a 30-inch cœlostat telescope. With this apparatus it should be possible to photograph the spectra of the brightest stars with a dispersion equal to that now employed for the Sun.

Spectra of Planets.—Excepting the objective prism, which can be used advantageously only with objects of small angular magnitude, the instruments employed in stellar spectroscopy can be equally well used in the study of nebulæ, planets, comets, and other heavenly bodies. The planets already afford a fruitful field of investigation, which promises to become more important in the future. As the light from the Sun reflected to us by the planets must pass twice through their atmosphere, the absorption thus produced can be detected in their spectra. In this way it has been found that Saturn possesses a dense atmosphere, which is absent or extremely rare on the rings. Perhaps the most interesting application of the spectroscope to the study of planets is the determination of their period of axial rotation. By proving spectroscopically that the matter composing the inner edge of Saturn's rings completes a rotation about the planet in a shorter time than the matter at the outer edge, Keeler showed observationally, as Maxwell long before had done theoretically, that the rings must consist of discrete particles. As soon as more powerful instruments become available, the disputed question of the rotation period of Venus will doubtless be settled by spectroscopic means.

Spectra of Nebulæ.—Comparatively few of the nebulæ have been investigated spectroscopically, and a most important field of research lies open here. The discovery of the terrestrial counterpart of the chief nebular line, the chemical and physical differences which characterize various parts of the same nebula, and the corresponding differences of radial velocity, are among the problems that most readily suggest themselves. The certain nature of the spectrum of such an object as the Great Nebula in Andromeda should be ascertained without further delay.

The Sun.

A complete knowledge of the phenomena and physical constitution of the Sun is of fundamental importance in connection with the

problem of stellar evolution. Every star in the universe is a sun, resembling in many respects the central body of our own system, but each representing a certain degree of development, from the nascent state, in close connection with the nebulæ, to the period of decline, exemplified in the red stars. With the exception of our own Sun, all of these stars are so distant from the earth that they appear in the most powerful telescopes as mere points of light. But with good atmospheric conditions a sharply defined image of the Sun a meter in diameter can be obtained, thus permitting the bright and dark spots upon its surface, and the flames at its edge, to be studied in their most minute details. A knowledge of the laws governing the variations of these phenomena is derived in part from statistical studies of photographs.

Solar Statistics.—The Royal Observatory at Greenwich, aided by auxiliary stations in India and Mauritius, has maintained a daily photographic registration of the visible phenomena of the solar surface on a scale of about 20 centimeters to the Sun's diameter. The data obtained from these plates, published annually in the *Greenwich Observations*, are sufficient to provide for investigations of the more conspicuous photospheric phenomena. These are supplemented by the "Sun-spot numbers," derived from the visual observations of many amateur and professional astronomers, which are published in the *Astronomische Mittheilungen*, established by Wolf, and continued by Wolfer, his successor at the Zurich Observatory. The prominences are observed visually with small instruments at Rome, Catania, Kalocsa, and Odessa, and the results, published regularly in the *Memorie della Società degli Spettroscopisti Italiani* and elsewhere, are suitable for a study of the distribution of the prominences in heliocentric latitude and longitude throughout the Sun-spot period.

In addition to prominences and spots which can be seen at the telescope, with or without the aid of the spectroscope, there are other solar phenomena of which a daily record is no less desirable. Scattered irregularly over the Sun's surface there are extensive areas of very hot calcium vapor, some of which coincide with faculæ. Though invisible to the eye, these regions can be photographed with the spectroheliograph, which records all phenomena characterized by brilliant emission of the K line of calcium. Thus the chromosphere and prominences also appear on suitably exposed plates taken with this instrument. A daily record of solar phenomena was maintained with the spectroheliograph at the Kenwood Observatory,

in Chicago, from February, 1892, to May, 1895, after which the instruments were removed to the Yerkes Observatory. On account of the necessity of remodeling the Kenwood telescope for other observations, this series of observations has been discontinued. Many photographs of the Sun have been taken, however, with a spectroheliograph attached to the 40-inch Yerkes telescope, and a daily series will soon be resumed as part of an extended programme of solar investigations, for which the necessary instruments are nearly completed. Spectroheliographs are also in service at the astrophysical observatories of Meudon and Potsdam, and a committee of the Royal Society is about to have one constructed for use in India.

Janssen has obtained directly enlarged photographs of Sun spots and photospheric granulations on a scale great enough to show the structure admirably. This work is still in progress at the Meudon Observatory, and some equally good results have been obtained by Bélopolsky at Pulkowa. It is important that further experiments of this kind be undertaken at other observatories, and that a systematic record of the changes in spot and atmospheric structure be maintained.

It will be seen from what has been said that when all of the new instruments are in use, the provision for recording the forms of solar phenomena will be nearly sufficient. The photographic record of Sun spots made under the direction of the Greenwich Observatory need not be duplicated elsewhere, at least in so far as the systematic reduction of all the plates is concerned. For comparison with spectroheliograph plates, however, direct photographs should be taken at other observatories. The small size of the solar image used at Meudon and at Potsdam (and planned for the Indian spectroheliograph) precludes the possibility of photographing the smaller details of the chromosphere. For this reason a large scale record to supplement that of the Yerkes Observatory should be provided for at some tropical station. For studies of special phenomena an even larger image could be used to advantage, and large-scale direct photographs of spots should be taken for comparison. The work of photographing prominences simultaneously in lines of different elements, such as calcium and hydrogen, which has already yielded some interesting results at the Kenwood Observatory, should also be continued. There are many other special investigations to be made with the spectroheliograph.

Spectroscopic Observations.—The solar spectrum has been studied with epoch-making results by Rowland and his associates at the Johns Hopkins University, and in the infra-red region by Langley and Abbot at the Smithsonian Astrophysical Observatory. The marked success of Higgs in photographing the extreme red on plates prepared by himself points to the possibility of extending this work to the infra-red, a result greatly to be desired. All of this work relates to the integrated light of the Sun, and not to any particular point on its surface. Our knowledge of the spectra of Sun spots is derived almost entirely from visual observations made with the spectroscopes of twenty years ago, and is purely qualitative in character. An adequate study of the characteristic widened lines, which may involve the application of photographic methods, should be based upon measurements of both wave-length and intensity. A large solar image and a powerful spectroscope are indispensable for this work. The following programme for the study of Sun spots may serve to illustrate the opportunity for research in this field:

	Size of image.	*Purpose.*
(1) Photograph of form (direct)...	17 cm......	To identify spot and give general form and heliographic position.
(2) Photograph of form (enlarged)	40 to 100 cm.	To record details of structure.
(3) Photograph of form (in calcium light).	17 cm......	To show distribution of calcium vapor for comparison with (1).
(4) Photograph of form spectrum..	17 cm......	To record widened lines.
(5) Photograph of form (H and K lines, with very high dispersion.)	17 cm......	To show radial motion of calcium vapor.
(6) Heat measurement with bolometer or radiometer.	40 to 100 cm.	To give heat radiation, compared with that of neighboring photosphere and center of the Sun.

The slit used in photographing the spectrum and in measuring the heat radiation is to be set successively at various points in the spot, which are recorded on the photograph of its form.

It would be easy to name many similar investigations of spots, faculæ, and prominences, which would be certain to add greatly to our knowledge of the Sun. It is perfectly feasible, for example, to study some of the most important phenomena of the reversing layer without an eclipse; indeed, many lines which are apparently too faint to be photographed at eclipses in the spectrum of the "flash"

have been repeatedly observed at the Yerkes Observatory in full sunlight. It should not be forgotton, moreover, that the prismatic camera or objective grating, though perfectly adapted for eclipse work, may give results which are not always capable of certain interpretation. Thus a faint chromospheric arc, whose depth is sufficient to require some time for the Moon to pass over it, may produce in a photograph an effect similar to that given by a much shallower but more brilliant stratum. With sufficiently powerful apparatus such questions could be easily settled by observations made in full sunlight. A large grating spectroscope (for many such purposes the astigmatism of the concave grating need not seriously interfere with its employment), used with a cœlostat and a long focus objective or speculum giving a large focal image of the Sun, would probably bring to light many new phenomena, and permit those now known but not yet understood to be rigorously investigated.

From recently published photographs it appears that at rare intervals the Sun's reversing layer is temporarily so completely changed in character as to render the solar spectrum corresponding to the disturbed region almost unrecognizable. In order to secure observations of other such phenomena—the only one so far recorded lasted only a few moments and involved a region whose length was at least one-sixth of the Sun's diameter—the solar spectrum corresponding to the most active regions of Sun-spots should be kept continuously under observation during the spot maximum. A series of photographs of the spectrum, taken at brief intervals of time, may be needed for this purpose. On account of the large number of photographs required to give a fairly continuous record, some plan of coöperation ought to be adopted. A single photograph of such a phenomenon would well repay all the time and trouble required to obtain it.

As the field of solar research is almost unlimited, many other problems requiring investigation will suggest themselves. If modern methods had already been applied to the solution of solar problems, thus diminishing the chances of securing new and striking results, a reason, though an inadequate one, for the present neglect of solar research might be found; but, with a few notable exceptions, such applications have yet to be made.

The Advantages of Reflecting Telescopes.

During the nineteenth century the development of the telescope in all countries except England was confined almost exclusively to

the refractor. In England the evident theoretical advantages of the reflector and the comparative ease of making large speculæ led during the first half of the century to the construction of larger and larger instruments of the reflecting type, which culminated in Lord Rosse's great six-foot reflector, erected at Parsontown in 1845. The crudeness of the mounting provided for this great mirror, due to the necessity of constructing it without the advantages afforded by modern engineering methods, did not prevent the Irish astronomers from securing many important results; but even had sensitive photographic plates been available at that time, the advances made in later years could not have been achieved, for a large mirror or objective, however perfect, is of comparatively little value unless provided with a mounting and a driving clock of accurate workmanship and suitable design. The absence of such mountings undoubtedly delayed for many years the recognition of the advantages of reflecting telescopes.

After the completion of Lord Rosse's reflector, attention seems to have been concentrated in large measure on the development of refracting telescopes. Notable exception should be made of Sir William Huggins, who fully perceived the advantages of the reflector in spectroscopic research and employed such an instrument exclusively in his classic investigations; but through the increased skill of the makers of optical glass and the genius of such men as Alvan Clark, the refracting telescope grew rapidly in size and perfection. The optical improvements were accompanied by corresponding advances in mechanical design, and great perfection has been attained in such modern instruments as the great refractors of the Potsdam, the Lick, and the Yerkes observatories.

Meanwhile the introduction of photography into astronomy and the rapid improvement of photographic processes had revolutionized observatory methods. In a period of rapid development the merits of the reflector, so obvious from an astrophysical standpoint, could not remain long overlooked. In 1883 Draper secured with a refractor the first photograph of the Great Nebula in Orion. The first to make extensive use of the reflector for the photography of nebulæ was Roberts, whose results form a record of great value. The construction of large reflectors was again undertaken. Special reference must be made to the work of Common, who not only constructed mirrors of three and five feet aperture, but provided them with improved, though hardly adequate, mountings and invented the double-slide plate-carrier. This simple device, which so greatly

reduces the difficulty of maintaining the image of a celestial object at a fixed point on the photographic plate, has proved to be of great importance. With its aid Common secured excellent photographs of the Great Nebula in Orion.

The high opinion of the possibilities of the reflecting telescope which is entertained by astronomers at the present time is based in large degree upon the results obtained by Keeler with the Crossley reflector of the Lick Observatory and by Ritchey with the two-foot reflector of the Yerkes Observatory. The Crossley reflector, with three-foot mirror by Grubb, is not provided with a thoroughly modern mounting.* In ordinary hands it is not improbable that photographs representing no marked advance would have been obtained with it; but through Keeler's skill the instrument was made to yield results of unexpected excellence. Few spiral nebulæ had previously been known to exist, but Keeler's photographs showed them to be as numerous as all other forms combined; indeed, they probably represent the type object to which our present ideas of the nebular hypothesis must be made to conform. Without the aid of a large reflector in competent hands it is doubtful whether this fundamentally important discovery would ever have been made.

Ritchey's results, though obtained with an instrument of only two feet aperture, constructed in the shops of the Yerkes Observatory, leave no element of doubt as to the great possibilities of the reflector. With this small instrument stars too faint to be seen or photographed with the 40-inch Yerkes telescope (the largest refractor hitherto constructed) can be photographed in forty minutes. The faintest star within reach of the 40-inch telescope is of the seventeenth magnitude, but with the two-foot reflector photographs made with exposures of six or seven hours show stars which are estimated to be one or two magnitudes fainter. The advantages of the reflector are still more striking in the case of the nebulæ, especially for such objects as the Great Nebula in Orion, the Great Nebula in Andromeda, the nebulæ in the Pleiades, the spiral nebulæ—indeed, for all such objects except the minute planetary nebulæ, which would also be shown to advantage by a reflector of great focal length. Furthermore, measurements of stellar photographs made with the Crossley three-foot reflector show that if the field employed is not too large the positions of stars can be determined with precision. For spectrographic research the reflector offers great advantages over the refractor, especially for work in the hitherto almost unex-

* Such a mounting is about to be constructed.

plored region of the ultra-violet. For these reasons astronomers are agreed that the construction of a large reflecting telescope at the present time would open up many fields of investigation, in which results of great value would undoubtedly be obtained.

Such a telescope, which would greatly surpass all existing instruments in power, should be used first in the northern hemisphere, and subsequently south of the equator, where the southern heavens, as yet unexplored with comparable optical power, offer limitless possibilities of discovery and research. With it there could be obtained a series of large scale photographs of the nebulæ, which would serve as standards of reference in years to come for the detection of evidences of motion or development. In conjunction with spectrographs of moderate and high dispersion, the latter mounted on fixed piers in constant-temperature laboratories, it would be possible to study stellar spectra and to trace the evidences of stellar evolution in a manner quite beyond existing means. Variable stars, which at their period of minimum are too faint to be seen in the most powerful instruments, could be followed through all their phases. The spectra of variable stars, which have been but little studied, could be investigated to the greatest advantage. Another important use of a large reflector would be in the measurement of the heat radiated from the stars. For this work a refracting telescope is useless, but the researches on stellar heat by Nichols at the Yerkes Observatory with a small reflector shows that much could be done with a large instrument of this type.

Respectfully submitted.

GEORGE E. HALE.

APPENDIX D TO REPORT OF COMMITTEE ON ASTRONOMY.

THE QUANTITY AND NATURE OF SOLAR RADIATION.*

BY S. P. LANGLEY.

The study of the Sun may be pursued from two standpoints, in a measure distinct from one another. On the one hand we may regard it as the nearest and therefore most readily studied of the stars, and seek to know its materials and their arrangement and reactions, in order to apply the results so gained to enlarge our views of the

* Prepared in answer to request from Advisory Committee on Astronomy of the Carnegie Institution, May, 1902.

universe; and, again, we may study the Sun as the source of that radiation which makes the Earth habitable. In this case we have a problem which concerns all humanity and all life on Earth, and one, however, strangely neglected, which is incomparably the most important to man of anything which Astronomy has to offer.

Our concern in this case is to determine how large an amount of radiation the Sun emits; what is the nature of these rays; what modifications they undergo in their passage through the Sun's envelope and the Earth's atmosphere; what, if any, variations occur in the total radiation of the Sun, and all this with final reference to the effects of these variations on life upon the Earth. The abstract and the utilitarian interests are closely joined, but it is chiefly with reference to the latter that the following remarks are offered.

The total radiation of the Sun, usually measured by the heating effect of the rays falling perpendicularly upon a square centimeter of area, has been determined by numerous observers at many stations on the Earth's surface. Many of these measurements have been corrected according to more or less plausible theories of the absorption of the Earth's atmosphere. The result so reached, which purports to be the heat equivalent of the solar radiation falling perpendicularly upon a square centimeter of surface outside the Earth's atmosphere, is termed the "solar constant." Whether or not the name is well chosen is doubtful, for we do not yet know, after a hundred years of careful actinometry, even within wide limits, whether the emission of the Sun is constant or variable. Indeed, if we should trust the experimental results implicitly, we should regard the "solar constant" as one of the most variable things in nature; for the most authoritative values range from less than 2 calories up to over 4 calories per minute.

The great discrepancy in these results does not necessarily imply an actual variation in the amount of solar emission, but is chiefly caused by differences in the method of reducing the observations to eliminate the absorption of the Earth's atmosphere. Thus it was for a long time customary to treat the atmosphere as if one portion of it were just like another in its absorption. Upon this basis, if, for example, a layer thick enough to exert a barometric pressure of 1 decimeter transmitted A per cent of the incident beam a second

* To prevent confusion, it may be stated that a solar constant of 3 calories per minute indicates that the heat equivalent of the solar radiation falling perpendicularly upon a surface of 1 square centimeter area for 1 minute of time is 3 times the amount required to raise the temperature of 1 gram of water through 1 degree centigrade.

layer exerting the same pressure would transmit the same percentage A of the beam, so that after passing the two layers the percentage A^2 would remain. This argument, which was that of Pouillet, leads to serious error, for it is now a well-determined fact that the layers of the air do not possess uniform properties of absorption and do not possess a uniform transmission coefficient for all wavelengths.

It has thus far proved, and, so far as can be seen, always will prove, impossible to determine from near sea-level with any precision by any observations, however careful or long continued, the "constant" of solar radiation. There is no good way to eliminate the complex effect of atmospheric absorption except to observe at the highest practicable altitude, preferably near the tropics, but most certainly in a dry and clear atmosphere, and preferably where there are two stations in view of each other, the first of which is at a notably greater altitude than the second, though the latter is itself at least some thousands of feet above sea-level.

Temporary expeditions with meagre outfits have gone from time to time to high mountain stations for solar observations, and small meteorological stations have even been longer continued. What is needed is rather a permanent astrophysical observatory equipped with the most powerful and refined modern apparatus for solar research and located at the highest and clearest station it is practicable to occupy.

The remarkable complexity of atmospheric absorption has been hinted at in what has just been said, but much remains unsaid and even unknown in this connection. Hardly less importance attaches to the absorption of the air for the radiations emitted by the Earth than to its absorption for the rays received from the Sun, and this part of the subject has been relatively unknown and till recently beyond the possibility of thorough study. It is apparent, however, that the nice balance of the receipt and outgo of radiation by the Earth is that which maintains the range of climate under which we live, and that a thorough understanding of the matter requires the study of the outgo as well as that of the incoming of radiation.

It is for this purpose and for extending our knowledge of the direct absorption of the solar rays in our atmosphere, and even largely for the determination of the solar constant itself, that the station at a relatively low altitude, but within sight of the elevated station, and which should be provided with a duplicate set of apparatus for energy of radiation work, is needed.

It has been surmised within recent years that the amount of radiation of the Sun is variable, with an average period of approximately eleven years, corresponding with the period of maximum frequency of Sun spots. It has thus far been impossible to determine this, and the possible change in the solar atmosphere, because of the variability of absorption of the terrestrial atmosphere. This subject should form an important part of the investigations to be undertaken by a supposed high-altitude observatory. The writer has long believed and said that a variability of the absorption of the solar envelope is a probable cause of the (probable) variability of the solar radiation. Investigation of the solar atmosphere should therefore go on at the same time as that of the Earth, and this incidentally gives another need for the high station, for this part of the work demands such a large and motionless solar image as can rarely be obtained in our lower atmosphere.

I have said little of the instrumental means for these principal objects, but they would be largely fitted to thermal studies. These studies would be associated with daily photographic records of the face of the Sun, and perhaps magnetic records and investigations into the emanation of X-rays and others. The barest suggestions of what may be done in the illimitable field are here given.

To conclude, these studies are utilitarian in the highest sense, for though we may never hope to affect the original source of solar radiation by any human effort, there is every hope that we may learn to forecast its effects upon the earth and provide for them.*

S. P. LANGLEY.

APPENDIX E TO REPORT OF COMMITTEE ON ASTRONOMY.

PRESENT STATE AND NEEDS OF ASTRONOMICAL RESEARCH.

BY SIMON NEWCOMB.

Of the two great branches into which astronomical research is at the present time divided, astronomy proper and astrophysics, the latter has been so fully treated in the papers submitted by Professors

* See letter of February 28, 1902, addressed to Mr. C. D. Walcott, Secretary of the Carnegie Institution, printed on page 110.

Pickering and Hale that it need not be considered in the present one. The following remarks therefore refer mainly to the branch of celestial measurement sometimes termed *astrometry*.

It must not be inferred that these two branches are really distinct. The forces of both have to be combined in working out the great problems of astronomy.

At the outset it is necessary to consider certain features in which astronomy is radically different from other branches of science, and which render it inadvisable to apply to its promotion any general rules derived from experience in the case of other sciences.

I. General Character of Astronomical Research.

Astronomy differs from other physical sciences in being founded entirely on observations, while the others rest mainly upon experiment.

Its laws and phenomena are of slow development, generally in cycles of years, centuries, or ages.

Their complete investigation involves difficult and complex mathematical problems, the numerical computations connected with which are long and arduous.

For all these reasons important astronomical results generally require a combination and comparison of observations continued through long periods of time.

In its relation to the welfare of mankind astronomy is also peculiar. In addition to its immediate connection with that welfare, astronomical research may bring to light cosmical processes pregnant with the destiny of our race.

These same circumstances render a different kind of genius necessary in the case of astronomy and of the sciences of experiment. The skill required in an astronomical investigator is not that of the experimentalist, but of one having such a grasp of the mathematical relations involved in the subject which he is treating that he can devise methods which most economically and surely lead to the desired results.

In working out these results a great amount of computation or observation is generally required. This part of the work can generally be done by routine computers, who are not necessarily masters of the great problems of astronomy, but who simply follow the formulæ supplied them.

II. Two Features of Astronomical Research at the Present Time.

The first feature is a certain one-sidedness of astronomical research as it has hitherto been pursued. Excluding private establishments, here and there, managed by their owners—institutions for instruction only, and inactive observatories—there remain fifty or sixty observatories of a more or less public character, supported by governments, universities, or special foundations, supposed to be in a state of astronomical activity. A rude estimate of the probable annual cost of these institutions foots up more than half a million dollars, and might approximate to a million. They are concerned almost entirely with the making, reduction, and publication of their current observations, while the important work of combining these observations and those made by our predecessors, so as to obtain from them the best ultimate results for astronomy, is comparatively neglected. The few institutions which do more or less work in this line occupy themselves mainly with special branches, and not with any comprehensive combination of the whole. Moreover, a comprehensive discussion and combination of the observations of the past is beyond the power of one man or of any existing organization.

The other feature is that the astronomical agencies of the world are independent organizations, and that each has generally worked in its own way with little reference to what is being done by others. At present there is a marked tendency to coöperation and unification, but these processes are slow.

The same remark applies to the mathematical researches necessary to the development of the science. They are made sporadically by isolated individuals, each using the method which he chances to have worked out or learned.

In view of recent progress toward international unification and coöperation, the time seems ripe for a general scheme for deriving the best results from the astronomical work of the past and present.

To gain the highest utility from a given amount of labor it is necessary that the latter be applied upon a comprehensive plan devised with especial reference to the ends ultimately in view. The following survey of the field may be regarded as an attempt toward the conception of such a plan. Emphasis will be laid on those subjects which require combination and coöperation, while subjects which individuals are able to deal with will not be dwelt upon.

III. The Researches Now Most Needed.

The heavenly bodies, to the investigation of which astronomical research is directed, are of two classes, the one comprising the bodies of the solar system, the other the fixed stars.

With respect to the first, it may be said that such subjects as the phenomena presented by the sun and the question of the physical constitution of this body, the aspects and rotation of the planets, the nature of the comets, and celestial physics generally either require only individual effort or are, in most cases, adequately provided for. Cases in which an investigator is in urgent need of more help than he commands can be dealt with singly as they arise.

In what concerns the motions of the planets it is believed that the existing tables satisfy all requirements.

The case is quite different with the satellites. The want of new tables of the moon is one of the most urgent in our exact astronomy, and one of the most difficult to supply. This difficulty arises from the two facts that the problem of the moon's motion is the most complicated of mathematical astronomy, while the amount of labor involved in carrying out any solution of it is greater than in the case of any other heavenly body. The promotion of the necessary preliminary research and the construction of new tables of the moon's motion, therefore, seem to me of the first order of importance.

Tables of the satellites of Jupiter are also greatly needed. It is believed that one, or perhaps two, competent astronomers are ready and desirous to undertake the preparation of such tables, but are deterred by the amount of labor involved and the difficulty of the problem.

Most interesting problems are afforded by the satellites of Saturn and Mars. It is believed, however, that these can be adequately dealt with by individuals, and therefore need no promotion by the Carnegie Institution. The same remark will apply to the satellite of Neptune. The writer is engaged in a discussion of the observation of this body, but needs no additional assistance.

A question of transcendent importance in astronomy is whether the force of gravitation varies exactly as the inverse square. There is now strong reason to suspect that such is not the case. If a deviation can be fully established, its explanation may open up a new field in physics. The work bearing on this question, which is not in regular progress, and yet requires most urgently to be done, is a

comparison of the best tables of Mars with all the good existing observations of that planet. This work is beyond the power of an individual, and is, therefore, of the class which can most properly be supported by an institution.

Passing now to the fixed stars, the foundations of a new science of stellar statistics are being laid. The pursuit of this science requires a combination of all the observations and methods of stellar research in both the fields of astrometry and astrophysics. To the very able and comprehensive survey of the subject by Professor Boss I wish to add the following considerations:

It seems to me essential that whatever aid the Carnegie Institution may give to researches in this class should be so applied as most effectively to gain the desired end. Among these ends that which must take the first place, so far as quantity of labor and need for help are involved, is the determination of the motions of as many stars as possible in every part of the sky. For this purpose it is essential that the researches of Professor Boss be supplemented by a re-reduction of certain series of former observations. The necessity of this arises from the fact that the proper motion of a star can, in most cases, be determined only by a comparison of observations as widely separated as possible.

The observations which most need re-reduction are the following:

1. The observations made at the Royal Observatory, Greenwich, by Maskelyne, from 1765 to about 1816. These observations have been neglected, owing to their having been made with an imperfect instrument, but it is believed that, with but little labor, they could be so reduced as to be made useful to astronomy. It would seem, in fact, that the office of the American Nautical Almanac was able to reduce most of them in a provisional way with the very limited means at its disposal. But it is desirable to have the work carried through more thoroughly.

2. The Greenwich observations by Pond, especially those of declination, have been shown by Chandler to be of the highest degree of precision. Those which he has provisionally reduced already form a valuable contribution to exact astronomy. It seems desirable that the work of reducing them should be completed.

3. The observations by Piazzi at Palermo, about the end of the eighteenth and beginning of the nineteenth century, are celebrated in the history of astronomy. Their re-reduction has been one of the desiderata of astronomy for half a century and the necessity for it has frequently been pointed out. Quite recently the work has been

seriously undertaken by Professor Porro at Turin and Dr. H. S. Davis in this country. The latter has been aided by the Trustees of the Gould fund. The question whether anything to promote the work can be done by the Carnegie Institution is respectfully submitted.

4. Attention has recently been called by Professor Rambaut to a remarkable series of observations made by Professor Hornsby at the Radcliffe Observatory, Oxford, beginning in 1774, which he is desirous to reduce. From his report it would seem that the results may be of special value. It therefore seems desirable to investigate the question of their precision and of the character of the results to be derived from them. When this is done the question of providing for their complete reduction could be considered.

Respectfully submitted.

SIMON NEWCOMB.

WASHINGTON, *October 8, 1902.*

APPENDIX F TO REPORT OF COMMITTEE ON ASTRONOMY.

PROGRESS AND PRESENT STATE OF CELESTIAL PHOTOMETRY AND PHOTOGRAPHY.

BY E. C. PICKERING.

CELESTIAL PHOTOMETRY.

The earliest estimates of the relative brightness of stars are recorded by Ptolemy in the *Almagest*, but careful comparisons, capable of revealing small changes in brightness, were first made by Sir William Herschel. The inadequacy of visual estimates was recognized by Sir John Herschel while engaged in extending and improving his father's work. This led to the constructions of the first stellar photometer, by which the light of a star was compared with a minute image of the Moon, adjustable in distance from the eye. The full possibilities of comparisons made without instrumental aid for large numbers of stars were realized by Argelander, whose *Uranometria Nova*, giving the approximate magnitudes and places of 3,256 stars, was published in 1843. This was followed twenty years later by the monumental *Bonner Durchmusterung* giving the positions and magnitudes of 324,189 stars in the northern heavens, in the prepa-

ration of which Argelander was aided by Schönfeld and Krüger. Schönfeld's *Southern Durchmusterung*, Thome's *Cordoba Durchmusterung*, and Gill and Kapteyn's *Cape Photographic Durchmusterung* have done a similar and even greater service for the southern heavens. But neither visual estimates nor photographic determinations can supply the precise measures of stellar magnitudes obtainable with the aid of a suitable photometer.

Reference has already been made to Herschel's stellar photometer, with which 69 stars were observed. These results are valuable mainly as pioneer efforts. But the measures of 208 stars made during the years 1852–1860 with a Steinheil prism photometer by Seidel were of a higher degree of precision. Zöllner's photometer, in which an artificial star is reduced to the intensity of the observed star by the aid of a polarizing prism, is described in his *Grundzüge einer allgemeinen Photometrie des Himmels*, published in 1861. This work also contains photometric observations of over 200 stars, which were made more for the purpose of testing the photometer than for the formation of a catalogue of magnitudes. Zöllner's photometer was first systematically used for this purpose at the Harvard College Observatory by Peirce, whose catalogue gives the magnitudes of 495 stars lying between + 40° and + 50°. Wolff's two catalogues (1877 and 1884) of stellar magnitude determined with a Zöllner photometer contain over 1,100 stars. Reference should also be made to the excellent work of Lindemann on the magnitudes of stars in the Pleiades and his revision of the magnitudes of the Bonn *Durchmusterung*, and to that of Ceraski on the magnitudes of circumpolar stars.

The first observatory which made the measurement of the light of the stars an important part of its work was that of Harvard College. In 1879 the meridian photometer, an instrument devised with special precaution for the elimination of systematic errors, was used to measure the light of 4,260 stars. Among them were included all stars which were of the sixth magnitude or brighter, according to any well-known authority, and north of declination — 30°. When published in 1884 it included the reduction to the photometric scale of the magnitudes of all the stars contained in the principal catalogues preceding it. The *Almagest*, A. D. 138, and Sufi, A. D. 964, were included, also the six catalogues of Sir William Herschel, which furnish determinations of the light of 2,785 stars a century ago with an accuracy not again attained for more than fifty years.

The *Uranometria Oxoniensis*, which contains the measures of 2,647 stars north of declination — 10°, made by Pritchard at Oxford with

a wedge photometer, was published in 1885. In 1886 Müller and Kempf at Potsdam entered upon their important investigations in stellar photometry. Their first work involved the precise measurement of the light of all northern stars in the *Durchmusterung* of the magnitude 7.5 and brighter. A Zöllner's photometer, embodying many improvements, was employed. The first portion of this valuable work, which was published in 1894, contained measures of 3,522 stars between declinations 0° and + 20°. The second portion contained measures of 4,416 stars between declinations + 20° and + 40°, and was published in 1899. Meanwhile, at the Harvard Observatory, a new and larger meridian photometer had been constructed, with which more than a million photometric settings have been made. All stars of the magnitude 7.5 and brighter in the *Durchmusterung* and north of declination — 40° have been measured, and the stars of the *Harvard Photometry* remeasured; also about 17,000 stars, mainly of the eighth and ninth magnitudes, arranged in zones at intervals of 5° in declination. All of this work has been extended to the South Pole by the establishment of a station in the southern hemisphere at Arequipa, Peru. The number of faint stars is so enormous that it does not seem advisable to attempt to measure them at all. The observations of Herschel show that slow changes of long period occur rarely, if at all, among the brighter stars. It is doubtful, therefore, if the meridian photometer need be kept actively at work, except for the purposes named below in connection with variable stars, and for measuring other standards. Of course, at intervals the brighter stars should be measured, and any tests of systematic errors affecting the scale established with this instrument will always be of value.

For measuring the faint stars much remains to be done. At the Harvard Observatory a 12-inch telescope has been mounted horizontally and a photometer attached to it, so that stars as faint as the thirteenth magnitude can be measured. The work is made purely differential by always measuring certain stars already measured with the meridian photometer. About 250,000 settings have been made with this instrument during the last four years. The photometric scale has been extended to magnitude 10.5 by measuring a series of zones of *Durchmusterung* stars, 10′ wide and 10° apart in declination. The photometric scale is now being extended to the faintest stars visible in the largest telescopes by the coöperation of the Yerkes, Lick, McCormick, and Harvard Observatories. By a small appropriation from the Rumford fund and the friendly coöperation of the

directors of these institutions, the use of telescopes of 40, 36, 26, and 15 inches has been secured for a single investigation. The two largest telescopes in the world are included in this work. The standards for measurement have been selected from the vicinity of known variable stars of long period. Charts of a large number of these regions have been constructed by Father Hagen, on which all the stars of the thirteenth magnitude and brighter have been entered. Thirty-six of these regions have been selected, approximately at equal intervals, but all north of declination —30°. In each of these regions five stars of the twelfth, fourteenth, fifteenth, and sixteenth magnitudes are selected as standards. The twelfth-magnitude stars have been measured with the twelve-inch telescope mentioned above. Good progress has been made in measuring the fainter stars with the larger telescopes. It is hoped that in two or three years we shall thus be able to furnish standards of magnitude for the faintest stars visible in the largest telescopes upon a uniform photometric scale.

It will be seen from the above statement that the photometry of the brighter stars is better provided for than are some other departments of astronomy. For the faint stars a beginning only has been made, and we have no means of judging whether the methods we are using are good. The use of one or more telescopes of at least two feet aperture is much to be desired. Other methods should also be employed to check systematic errors.

Various attempts have been made to determine the photographic brightness of the stars. The first method, that of measuring the diameter of the images, proposed by Bond in 1857, has perhaps been more generally used than any other. Methods have been tried depending on variation in aperture and exposure and on the use of polarized light. If this problem can be solved, it may supersede visual observations of magnitude. The fact that red stars photograph faint is rather an advantage, since a comparison with visual magnitudes gives also a definite measure of the color of the star. The only complete solution of the problem of determining the colors of the stars or other sources of light is by measuring the intensity of different portions of the spectrum. This may be done much more easily by photography than visually. One of the principal objects of photometry is to determine the distribution of the stars. Again, photography has a great advantage over visual observations, especially in the case of faint stars. If possible, the stars should be classified according to their physical condition as shown by their spectra. In nearly all of this work the ultimate comparison is the

estimate of equality by the eye. Many of the errors thus introduced may be eliminated by substituting for the eye a radiometer, bolometer, or thermopile and measuring the proportion of heat intercepted by an image on a photographic film. Another piece of work which is sadly in need of revision in accordance with modern methods is the precise determination of the relative brightness of the Sun and other members of the solar system as compared with the stars.

Variable Stars.—One of the most important applications of photometry is to the study of stars which vary in light. Variable stars may conveniently be divided into new stars, variable stars of long period, those having small and irregular variations, those of short period, and the Algol stars.

There is no obvious way of aiding the prompt discovery of new stars, and when such an object is found the general interest in it insures an abundance of observations. Except in the case of stars bright enough to be visible to the naked eye, all recent new stars have been found by the aid of photography, and nearly all of our knowledge of them has been found by photographic processes.

The study of variable stars of long period is one that can be greatly aided by judicious appropriations. The laws regulating the changes in these bodies are unknown and appear to be complex. They can be determined only from long series of observations extending over many years. If not made now we may be blamed by future astronomers who may justly say, "How much we could do now if our predecessors had not neglected their opportunities." Each star should be observed at least once a month. The observations are easily made, require but little apparatus in addition to a telescope, and much valuable work has been done on them by amateurs. Many of the large telescopes of the country are now idle during a great portion of the night, for want of the means of employing a competent observer. A small sum would pay the salary of an advanced student who might thus at the same time continue his studies and do useful work. Careful superintendence and coöperation would be required to insure a proper distribution of the observation of each star, otherwise as at present some stars would be neglected, others observed more frequently than is necessary. The same comparison stars, the same system of photometric magnitudes, and the same form of publication should be used by all the observers.

The observations of the variable stars of short period stand on a wholly different basis. These stars appear to vary with perfect uniformity, except for slight variations in the length of the period. It

is therefore only necessary to determine once for all the nature of the variation, and then occasionally determine the times of maximum light to see if the period has undergone any change. The variations can be determined so accurately by photometric measurements that visual observations are of little value. The light curves of a large part of them have already been determined at Harvard, and it is now only necessary to measure these stars from time to time. Some of them vary so rapidly that the periods of variation can be found with great accuracy. An attempt is made at Harvard to measure each star which changes more than half a magnitude an hour, on three nights each year when it is increasing in light, and on an equal number when it is diminishing. The same remarks apply with even more force to stars of the Algol type. If all the light curves can be determined once for all, occasional observations only will be needed later.

We may therefore conclude that appropriations are much needed to secure the required observations of variable stars of long period, but that sufficient provision is now made for the photometric study of other variable stars. Spectroscopic observations of such stars, which are greatly needed, are referred to in the section on spectroscopy.

Celestial Photography.

The application of photography to astronomy by Bond in 1850, and again in 1857, was an advance comparable in importance with the invention of the telescope. Bond's experiments were made with a visual telescope ill-adapted for the work. The production of the first photographic refractor by Rutherfurd in 1864 enabled him to obtain many remarkable photographs of the Sun, Moon, and stars. His negatives were used by Gould in 1866, and later by Jacoby and others in the precise measurement of such stellar groups as the Pleiades. In 1880 Draper obtained the first photograph of the Great Nebula in Orion. Up to this time the use of wet plates had greatly hampered the work, but the introduction of the much more sensitive dry plates led at once to important advances, especially at the Cape, at Paris, and at Harvard. At the Cape of Good Hope, Gill photographed the great comet of 1882 with an ordinary portrait lens strapped to a telescope. The immense number of stars recorded on the plate led to the publication in 1896 of the *Cape Photographic Durchmusterung*, which gives the approximate positions and magni-

tudes of 454,875 stars, nearly all south of declination — 18°, as measured by Kapteyn on Gill's plates. This great work is memorable not only as the first of its kind, but for the promptness with which it has been completed and published. The successful results of the MM. Henry at the Paris Observatory led to the formation of a permanent international committee for making a photographic chart of the sky. At the first international congress held in Paris in 1887, seventeen different nationalities were represented. A gigantic plan was formed in which eighteen observatories took part. Each agreed to have a 13-inch telescope constructed, and to photograph a certain zone with a short and a long exposure. A catalogue was to be formed from measures of all the stars shown on the plates taken with short exposures, giving about a million and a quarter stars to the eleventh magnitude. The long exposure plates, containing perhaps twenty million stars to the fourteenth magnitude, were to be published in the form of charts. The Potsdam Observatory has published two quarto volumes of about 500 pages each, giving the results of measurements of 57 and 38 plates. They give the precise rectangular coördinates of each star, but, owing to the great expense of the reductions and the lack of suitable meridian circle positions, the values of the right ascension and declination are only approximate. Three hundred of the charts have been published. The size is 28 x 28 centimeters and the scale 30 seconds to one millimeter. Eleven thousand of these plates would be required to cover the whole sky. Although fifteen years have elapsed since the first conference, the amount of material published by no means represents the amount of work done. No zone has been taken by any observatory in the United States. Including the very heavy cost of measurement and reduction, it is doubtful if the entire work could be completed at a total expenditure of $3,000,000. This great undertaking, if ever completed, will furnish a record of the state of the heavens upon which the work of centuries may be based. The plan adopted at the Harvard Observatory in the early eighties recom-, and mended to the first Paris conference in 1887, was that all the photographs should be taken with a single photographic doublet. Each plate taken with the Bruce 24-inch telescope, completed in 1892, which is of this type, covers a field of 25 square degrees, as compared with the four square degrees covered by the international charting telescopes. The errors in such plates due to distortion have been shown by Professor Turner to be insensible. The entire sky in either hemisphere can be charted with this instrument in one

or two years. The Draper telescope in Cambridge and the Bache telescope in Arequipa, Peru, are of this type, and have been in constant use by the Harvard College Observatory during the last thirteen years. The apertures are eight inches, and each covers a field 10 degrees square. The resulting collection of photographs represents each portion of the sky on from one to two hundred nights, and furnishes a historical map of great value. It has not seemed advisable to go to the expense of measuring and cataloguing a large portion of the fainter stars, but when an object of interest is discovered its history is always readily available, as has been shown in many striking cases. Prints on glass of any particular plate in this series are furnished to those who may require them for special studies. There can be no doubt that a systematic examination of these plates would lead to many important results.

Photography of Nebulæ.—The recent development of the reflecting telescope, referred to more specifically in another section of this report, has made it possible to secure with comparatively small instruments photographs of nebulæ greatly surpassing in delicacy of detail the best results previously known. This new era was inaugurated by the late Director Keeler of the Lick Observatory, whose discovery that the typical nebula is spiral in form, and that tens of thousands of these spirals are scattered over the heavens, is of fundamental importance in connection with the great problem of stellar evolution. In spite of the undoubted evidence of motion revealed in the very forms of these nebulæ, no actual change in their outlines has as yet been detected. It is very important that an extensive photographic campaign should be undertaken with a very powerful telescope, for the purpose of securing a permanent record of these nebulæ as they exist today. For comparison with photographs taken fifty or one hundred years hence such a series of photographs would be invaluable.

Photography of Moon and Planets.—Reference has already been made to the photographs of the Moon obtained on wet collodion plates by Rutherfurd. For many years no advance beyond these remarkable results was made, in spite of the advantages arising from the use of gelatine dry plates. It remained for the then recently erected 36-inch refractor of the Lick Observatory to give still sharper photographs in 1890. About the same time, in 1889, similar results were obtained by the Harvard astronomers, enlarging the image in the telescope. The most extensive series of lunar photographs hitherto obtained is that at the Paris Observatory, where Loewy and

Puiseux have used the great equatorial coudé for this purpose with striking success. Their great photographic atlas of the Moon, with the atlas prepared by Weinek from Lick and Paris negatives, provide much material for a study of lunar topography. The recent work of Ritchey with the 40-inch Yerkes telescope, a visual instrument adapted for photography by the use of a yellow color screen, has yielded remarkable photographs of the Moon, which should be of service in the study of the smaller details. The smallest markings perceived under good conditions at the telescope are yet to be recorded photographically, but in spite of this fact the best photographs now available are much superior to visual observations for some classes of lunar research.

Photographs of planets which are at all comparable with visual images have not yet been obtained, though the results of Common, the MM. Henry, Schaeberle, W. H. Pickering, and others give reason to hope that marked advances may be expected in the future. Judging from the results of recent experience, it is probable that the improvement of both lunar and planetary photographs will come through the use of larger focal images. An increase in the sensitiveness of photographic plates, unless it involved a corresponding increase in the size of the silver grains, would be of the greatest service in this and in most other departments of celestial photography. For this and other reasons the encouragement of research in photography, which might lead to the improvement of sensitive plates, is greatly to be desired.

Respectfully submitted.

E. C. PICKERING.

REPORT OF ADVISORY COMMITTEE ON PALEONTOLOGY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Paleontology would respectfully report as follows:

The principles upon which research in paleontology should be encouraged appear to be very similar to those in zoölogy. The problems and methods are different. It is not necessary, for example, to institute a permanent paleontological station.

First, it is very desirable to *encourage certain explorations*, especially such as are not provided for by any existing government or state institutions.

Second, the committee is very strongly of the opinion that a *more exact correlation of European and American horizons*, or the establishment of standard bench-marks for the geological time scale, is a matter of first importance as a problem. This is expanded under IV.

Third, a publication of the same character as the *Paleontographica* of Germany, or the memoirs of the Paleontological Society, is one of the greatest needs of American paleontology at present, as there are no institutions with funds for the adequate preparation of illustrations. A number of valuable memoirs have already been offered to the Institution.

Fourth, awards or special grants for paleontological research are desirable on the principles carefully considered and hereinafter expressed in sections I and V.

More in detail our recommendations are as follows:

I. *General principles.*—That a comprehensive plan for the encouragement of paleontological research extending over a number of years should be prepared, and that no special applications should be considered or acted on until certain general principles are established. Chief among these are (*a*) that it is desirable to aid existing institutions, not to duplicate their work, either in research or publication, but to strengthen and coöperate; (*b*) that diffusion of the advantages of the Institution throughout the existing centers of research will effect better results than centralization.

II. *Advisory committee.*—That, as a feature of the permanent plan of the Institution, an advisory committee of five on paleontology

be constituted on the rotation system, to represent as far as possible—

Vertebrate paleontology—fishes and amphibians.

Vertebrate paleontology—Birds, reptiles, and mammals.

Invertebrate paleontology—Paleozoic.

Invertebrate paleontology—Mesozoic and Cenozoic.

Paleobotany.

This committee to be chosen for terms of five years, but at the first election to determine by lot the members to hold office for one, two, three, four, and five years respectively, so that one member of the committee shall go out of office each year and not be eligible for immediate reëlection, but to be replaced by a new member elected by the Carnegie Institution.

Such advisory committee to hold stated quarterly meetings, for attendance at which members present will receive a fee in addition to expenses; to elect its own chairman annually, and to appoint annually a salaried secretary, whose duty it shall be to keep all the records, to prepare and present applications, and to act especially as editor of paleontological publications.*

The principle of rotation in office, both as regards the committee of five and its secretary, will secure the constant infusion of fresh blood and keep the committee abreast of the most recent advances in paleontology.

The organization, duties, and powers of this committee will naturally conform to those assigned by the Institution to advisory committees on other subjects.

III. *Personnel.*—That a full list of the active investigators of the country, including the younger as well as the older and better known men, should be prepared and kept constantly renewed, in connection with the specific problems in regard to which investigation from time to time seems to be most urgent.

IV. *The establishment of standard bench-marks for the geological time scale.*—It is recommended that the Advisory Committee on Paleontology be authorized (and in case no such committee is appointed, that a committee be appointed for this purpose) to undertake the establishment of a few carefully selected time bench-marks, which shall constitute a permanent set of standards for discriminating divisions of geologic time, and which shall serve as datum

* It is probable that the salaried secretary could also act as the secretary of one or more other committees, say the zoölogical, and be connected with the central office in Washington.

planes for the correlation of geological formations and events of other continents than those in which the standards are established.

The imperfection of the standards now in use arises from the fact that they were originally defined and established in pre-evolution times, before the reality of the gradual and continuous modification of the life of the globe was appreciated, and only the rocks between indefinite boundaries are defined by lists of their characteristic species, whereas it is now seen to be essential to determine the precise stage of evolution of the races whose representatives lived before as well as after any artificial time boundary which may be adopted as a standard.

The sections which must be used in establishing such time bench-marks are chiefly in Europe, and because they are within easy reach for personal examination, European geologists may not appreciate as do foreigners the necessity for their precise and full definition.

American geologists have been driven to appreciate the importance of these standard time bench-marks for the discussion and classification of geological problems, and to realize that accuracy in all discriminations of geological time values in America and throughout the world depends upon the establishment of a single set of standards with which to make accurate correlations of secondary standards in other continents.

The task of establishing such standards will involve comparative and international investigation, and it is therefore outside the legitimate field of the government Geological Survey of any particular country. It will involve also an amount of travel and work in countries foreign to the nvestigator's home which places it beyond the capacity of private enterprise of such paleontologists as would alone be competent to do the work.

It is probable that coöperation with foreign geologists, and perhaps with official state surveys, may be required in executing the task, and it is suggested that the *Congrès Geologique Internationale* is a convenient agency through which such coöperation may be attained.

Considerable investigation, correspondence, and possibly travel will be required before it can be definitely estimated what shall be the extent of the investigation and the amount of expense necessary to complete it.

It is therefore recommended that for the first year the Carnegie Institution provide the means for a small committee of experts, authorizing them to prepare a plan for carrying out the investigation herein described, but leaving the decision as to the amount of ap-

propriation to be allotted to the execution of the work until the committee shall make further report and recommendation.

V. *Awards of grants.*—That, with certain exceptions, it is desirable, instead of permanently detailing the investigators of the country to certain subjects, possibly to the detriment of the institutions with which they are connected, to establish quarterly, half-yearly, or yearly grants. These grants should be apportioned according to the importance of the subject or the ability of the investigator, and should be at least equivalent to the salaries for the same period received by the investigators from the institutions with which they are connected. In this manner teachers, curators, state and government scientists would be entirely relieved of routine work for certain periods, and would be enabled to devote their time exclusively to certain investigations, At the same time these institutions would be able to provide substitutes and the work in the various laboratories would not be interrupted, but materially benefited and enriched. The committee feels very strongly at the present time that the chief drawback to progress in discovery in paleontology is that the majority of the ablest men are so heavily burdened with administrative work that they do not enjoy the repose and opportunity necessary to research. At the same time the students and younger generation of the country would suffer if the ablest men were permanently withdrawn by the Institution.

VI. *Exploration.*—Paleontological exploration in general in this country is well provided for by the museums of New York, Chicago, Pittsburg, New Haven, Kansas, and the United States Geological Survey, among other institutions. More distant exploration, such, for example, as the Antarctic expedition proposed by Mr. J. B. Hatcher, should be supported on the side of paleontology as well as zoölogy, because likely to produce very important results.

VII. *Publication.*—In addition to the publication of such paleontological memoirs as are not provided for by existing agencies, we recommend the publication of a Journal giving brief statements and short summaries of the results of all paleontological investigations—a sort of "Comptes Rendus."

Respectfully submitted.

HENRY FAIRFIELD OSBORN, *Chairman*,
HENRY SHALER WILLIAMS,
Committee.

NEW YORK, *October 1, 1902.*

REPORT OF ADVISORY COMMITTEE ON ZOÖLOGY

CONTENTS.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The undersigned members of the Advisory Committee on Zoölogy respectfully present the following report. Professor Alexander Agassiz was originally a member of the committee, and the report embodies much of his valued advice. We greatly regret that other duties prevented his remaining on the committee until the completion of the report. Dr. C. Hart Merriam was prevented from attending the final meeting, and some of his views, expressed by letter, are inserted at the close.

Our recommendations are as follows:

I. GENERAL PRINCIPLES.

That a comprehensive plan for zoölogical research extending over a number of years should be prepared, and that no special application should be considered or acted upon until certain general principles are established. Chief among these are the following:

1. That it is desirable to aid other existing institutions; not to duplicate their work, either in research or publication, but to strengthen and coöperate. This is further explained under V.

2. That it is also desirable to promote certain independent explorations, expeditions, researches, and publications in the name of the Institution. This is further explained under V and VII.

3. That, with certain exceptions hereinafter mentioned, diffusion of the advantages of the Institution throughout the existing centers of research will on the whole effect better results than centralization.

II. Permanent Advisory Committee.

That as a feature of the permanent plan of the Institution, an advisory committee on zoölogy somewhat similar to this be constituted on the rotation system, to act as advisers in connection with the Marine Biological Laboratory and Experimental Station, the encouragement of research, expeditions, and all applications for grants, etc., in the field of zoölogy.

More in detail, that this advisory committee of five be chosen to represent the following subjects:

1. The zoölogy of vertebrates.
2. The zoölogy of invertebrates.
3. Embryology and cytology.
4. Experimental morphology.
5. Biology, œcology, evolution, heredity, biometrics, geographical distribution, etc.

This committee to be chosen for terms of five years, but at the first election to determine by lot the members to hold office for one, two, three, four, and five years, respectively, so that one member of the committee shall go out of office each year, and not be eligible for immediate reëlection, to be replaced by another member elected by the Carnegie Institution. For this purpose the committee shall submit annually a list from which the Institution may select the new member. In this manner the committee will keep thoroughly informed as to the past and will be freshened constantly by the accession of a new member each year.

Such advisory committee to hold stated quarterly meetings, for attendance at which members present will receive an adequate fee in addition to expenses.

Such committee to elect its own chairman annually.

Such committee to have the power given by the Institution to appoint annually a salaried secretary, preferably a scientific man, whose duty it shall be to keep all the records, to prepare and present applications, and to act especially as business editor of zoölogical publications.

The organization, duties, and powers of this committee will naturally conform to the rules adopted by the Institution for advisory committees on other subjects.

III. Personnel.

We recommend that a full list of the active investigators of the country, including the younger as well as the older and better known men, be prepared and kept constantly renewed, in connection with the specific problems in regard to which investigation from time to time seems to be most urgent.

IV. Marine and Experimental Stations.

While, as stated above, coöperation should be the main feature of the Institution, the committee strongly indorse the establishment of a permanent biological laboratory as a central station for marine biology in general, with branches at such other points as may seem desirable; also affiliated or independent experimental stations for the study of physiological zoölogy and problems relating to heredity, evolution, etc.

Since the publication of researches from the marine and experimental stations in the future will be so closely related to zoölogy in general, it would appear advisable to refer this general question to the permanent advisory committee on zoölogy.

As regards an experimental station, among the most important desiderata at present are experiments in heredity, in variation, in instinct, in modification, all of which should extend over a series of years and be planned systematically.

We would advise referring report on this matter to a committee of specialists, who are best able to present a permanent plan. Naturally consideration will have to be given first to the question whether the best results will be achieved by making this station a part of the marine station or placing it under separate direction and in another locality. Our main purpose now is strongly to recommend the establishment and endowment of such a station or stations.

V. Expeditions and Explorations.

As stated in the brief digest of applications received,* the whole scheme of exploration by the Carnegie Institution is admirably set forth, especially by Stejneger, Mayer, and Hatcher. All are of great interest and importance. These explorations involve very large sums of money and expenditures extending over many years.

* Here omitted.

VI. Special Grants.

With certain exceptions, it is desirable, instead of permanently detailing the investigators of the country to certain subjects, possibly to the detriment of the institutions with which they are connected, to establish quarterly, half-yearly, or yearly grants. These grants should be apportioned according to the importance of the subject or ability of the investigator, and should be at least equivalent to the salaries for the same period received by the investigators from the institutions with which they are connected. In this manner teachers, curators, state and government scientists would be eutirely relieved of routine work for certain periods, and would be enabled to devote their time exclusively to certain investigations. At the same time these institutions would be able to provide substitutes. The work in the various laboratories would not be interrupted, therefore, but materially benefited and enriched. The committee feel very strongly at the present time that the chief drawback to progress in zoölogy is that the majority of the ablest men are so heavily burdened with administrative work that they have no opportunity for continuous research; nor are they able to command the means for carrying on zoölogical researches at remote points. At the same time the students and younger generation of the country would suffer if the ablest men were permanently withdrawn from instruction by the Institution.

Exceptions will naturally arise in case of unattached investigators or of those detailed by the Institution for prolonged expeditions or researches.

As regards grants or subsidies for research, provision should naturally be made for visiting museums both at home and abroad, for the examination of collections, for supplying investigators with necessary books and apparatus from a central bureau, and with the means for carrying on special researches in the localities where the opportunities are to be found.

Investigators should enjoy considerable freedom of action. It frequently happens that while in the field unforeseen opportunities arise for new lines of work, and the most effective investigator is the one who knows how to seize such opportunities and make the most of them.

VII. Publication.

As regards publication, it is desirable to establish a central publication bureau of the Institution to regulate the issue of large *monographs in quarto* form and of *shorter papers in octavo* form. As suggested above, the salaried secretary of the Zoölogical Committee could also act as editor.

Zoölogical monographs treated from the anatomical, embryological, and biological standpoint, similar to the monographs issued by the Naples Station, are of great importance. The committee also recommend the preparation and publication of systematic monographs of groups in zoölogy, studies of faunal areas and comprehensive studies of the geographic distribution of life.

Without recommending in detail, it may be said that fully one-half of the applications for special grants for research and for publication are worthy of very careful consideration. They suggest work of just the character which the Institution, it appears, is especially designed to undertake and encourage.

The urgent character of these requests indicates that sufficient means are not yet provided in this country for publications of importance. At the same time, the committee are of the opinion that several of the above applications, together with a number of minor applications included in the general list, might more properly be provided for by the United States Fish Commission, the United States National Museum, the Boston Society of Natural History, the National Academy of Sciences, and other institutions.

In connection with the matter of publication, reference may be made to a letter dated April 28 from E. B. Wilson to Professor Walcott, in which a general scheme for zoölogical publication is outlined and discussed at some length.

VIII. Subsidies.

Cases may arise, such as that suggested by the editor of a well-known journal, where a subsidy might wisely be extended to a journal of great value, "the income from which from subscriptions, sales, etc., amounts to less than half the cost of publication, the balance having been made up by contributions from the editors and other outside sources." This, again, is a matter where some general principle of action must be adopted by the Institution before a recommendation can be made.

The Zoölogical Station at Naples will in all probability be one of the most important centers for special research work connected with the award of special grants mentioned under VI. It is therefore desirable, and this committee strongly recommend, that the Carnegie Institution subscribe annually for a table at Naples to the value of five hundred dollars ($500). Among the applications which will presently be received is one for a special line of investigation at this station.

Respectfully submitted.

HENRY FAIRFIELD OSBORN, *Chairman*,
EDMUND B. WILSON,
W. K. BROOKS,
Committee.

NEW YORK, *October 15, 1902.*

IX. SUPPLEMENTARY NOTES.

Extract from letter of Dr. C. Hart Merriam, dated June 7, 1902.

"While in accord with much of the report, I find myself more and more opposed to the plan of scattering the work and funds of the Institution. I am opposed, therefore, to the first of the general principles stated. It seems to me that, as a rule, existing institutions should be allowed to continue their work without aid or interference from the Carnegie Institution.

"Its affairs should therefore be conducted from the first with an appreciative sense of the dignity, unity of purpose, and continuity of effort essential in an organization which means so much for human progress. Its strength and influence should not be weakened by diluting and scattering its resources, but husbanded for uses in keeping with the promise and scope of the Institution. It is quite conceivable that its rich endowment might be so distributed as to partake of the nature of charities to individuals and institutions.

"For some time I shared the views expressed in the committee's report, but have gradually come to change my position in this matter. I am now fully convinced that the Carnegie Institution should carry on its own work, under its own name, and should publish the results in its own series of publications."

Note by E. B. Wilson.

I am of opinion that the regular support by the Carnegie Institution of at least two tables at Naples is highly desirable. The advantages derived by American biology as a whole from the Naples station in the past have been of incalculable value ; there is every reason to look forward to at least equal benefits in the future if sufficient opportunity be given.

There are at present three American tables at Naples—one supported by American women, for the use of women; one by the Smithsonian Institution, and one by private subscription. Both the latter are of uncertain tenure. Although payments for the last named are in arrears, Professor Dohrn has generously continued the table despite the existence of a considerable deficit for past years.

There are for the current twelve months more applicants than can be accommodated by all three tables combined.

Note by Henry F. Osborn.

The Concilium Bibliographicum at Zurich, under the direction of Dr. Herbert Haviland Field, an American, is worthy of an annual subsidy of not less than $500 for its catalogue and digest of all current zoölogical literature, invaluable to workers in every branch of zoölogy.

REPORT OF ADVISORY COMMITTEE ON PHYSIOLOGY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: During the last thirty years the greatest advances in our knowledge of disease have been made through bacteriology, *i. e.*, through the study of the life history of microbes. But it has now been shown that the microbes are injurious not by their simple presence in the body, but through the chemical products of their activity, and that antidotes for the disturbances thus produced are to be found in chemical substances formed in the animal body as the result of reactions set up in the tissues and fluids by the microbes or their toxic products. The study of these products and their antidotes must of course be prosecuted by the methods of physiological chemistry, and it is, therefore, by the aid of this science that we may expect the next great steps to be taken in the advancement of medical science. But in order that these steps may be firmly planted and lead to an accurate knowledge of those disturbances of function which constitute disease it is essential that the normal metabolism of the body should be much more thoroughly understood. Here, too, it is largely by means of physiological chemistry that advances must be made, and it is thus evident that it is more largely for the chemical than for the physical side of physiology that, in the immediate future, special provision for coöperative work should be made.

The most important problems of physiological chemistry are, perhaps, those which are connected with nutrition, and there is little doubt that the establishment of a well endowed laboratory, provided with all the apparatus needed for the study of the nutritive phenomena of the animal body, would in a few years lead to results of the greatest importance for the welfare of humanity. Such a laboratory would, however, have to be provided with certain forms of physical apparatus, for the study of the physical phenomena connected with nutrition should go hand in hand with that of the chemical processes.

A laboratory thus established and equipped would be something quite different from any existing institution, for though certain laboratories possess particular forms of apparatus adapted to researches of this sort, there is no place where the student of nutrition (in the broad sense of the word) can find under one roof all the special

appliances which he may need to employ in his researches. The question of the proper location of such an institution need not be discussed until the Board of Trustees has decided whether the establishment and equipment of laboratories is to be regarded as a part of the recognized work of the Carnegie Institution.

Another way in which it is desirable for the Carnegie Institution to aid in the advancement of physiological science is by assisting individuals of proved ability in their researches. This may be done—

(1) By supplying them with apparatus and material for study which they could not otherwise obtain.

(2) By affording them assistance, either clerical, laboratory, or editorial, thus relieving them of the drudgery of routine work and enabling them to expend their energies in directions in which they will be most effective.

(3) By making it possible for them to secure leave of absence from their work as teachers in order to devote themselves for a year or more to some special research, either in a laboratory already established or in the laboratory for the study of nutrition, the creation of which has been above suggested.

Your committee is of opinion that the demands made by modern methods of education in physiology and by the increase in the size of classes has caused some competent observers to give up all attempt at original work. For like reasons, men who enter laboratories as demonstrators and assistants are soon overwhelmed by the necessity of giving up all their working hours to instruction. Whatever is done to aid physiological research must insure to the worker full relief from the labor of educating students.

In the case of tried men, now too actively employed as professors, it will be advisable, when they desire to conduct researches of sufficient importance, either to secure easing of their work by assistance or to enable them for long enough periods to be free from all need to teach.

Respectfully submitted.

S. Weir Mitchell, *Chairman*,
H. P. Bowditch,
W. H. Howell,
Committee.

October 16, 1902.

REPORT OF ADVISORY COMMITTEE ON ANTHROPOLOGY

CONTENTS.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Anthropology has the honor to present the following report, which can not be regarded as more than preliminary, owing to the wide extent of the subjects to be considered, to various questions that naturally arise, such as those of infringement on territory already occupied, as well as to the difficulty of finding men specially qualified for certain divisions of research.

I. Papers Referred to the Committee.

The correspondence referred to the committee has been considered and brief notes characterizing the subject matter are attached to the communications in each case. Many of the papers are found worthy of careful attention, while a number relate to subjects not within the scope of the Advisory Committee on Anthropology. No grants to individuals or for special, limited researches are recommended, but rather it has been sought to organize the work on broad lines, as a basis for future elaboration in special lines. However, many suggestions made by correspondents have been here embodied.

II. The Field of Anthropology as a Whole.

The science of man covers a wide range of diversified subjects, which may be classified under a few general heads. Viewing the human kind or species as a whole, the science considers (1) its physical or biological characters under the head of *physical anthropology*; (2) its intellectual characters and history under *psychology*; (3) its arts and industries under *technology*; (4) its social structures and functions under *sociology*; (5) its languages and letters under *philology*; (6) its systems or opinion and its cults under *philosophy and religion*; (7) its æsthetical activities under *æsthetics*.

The study of these phenomena is carried on with two principal ends in view, viz., (*a*) to write the history of the race, (*b*) to discover the principles and laws of human development with the view of utilizing them to regulate the present and to mould the future of the race. As yet, these researches are hardly begun, because of the vastness of the subject, the inadequacy of financial support, and the limited number of students engaged in the work.

It is to be observed that a number of branches of the general subject have been highly specialized, and that some are, on this account, regarded as independent fields of research. Such are social science, political economy, and psychology. These branches your committee have considered only so far as every research that has to do with peoples and cultures must deal with all phases of the science of man. The special requirements of these branches have not been weighed, and no recommendations are made with respect to them. Essential coördination of the several branches can be considered when the reports of the various committees are compared.

Although, as above indicated, researches have been initiated in many parts of the anthropological domain, of none of the fields is the occupation complete, and in no part is it so vigorous as to satisfy the requirements of science. In this connection it may be further mentioned that the urgency of the demand for research is due not a little to the fact that primitive culture, in which are hidden the keys of history, is being rapidly destroyed by the spread of civilization, and that the peoples of the earth are fast losing their original race characteristics.

III. Present Researches in Anthropology.

American students have naturally and properly turned their attention mainly toward researches among the aborigines, and it seems advisable to continue for the present along the old lines.

The more important agencies at present actively engaged in anthropological researches may be briefly noted:

1. The Bureau of American Ethnology, established in 1879 for the investigation of the North American Indians, has latterly extended its field to cover the American continents and islands, so far as the law and its means permit. Its investigations refer particularly to the languages, social customs, beliefs, arts, and industries of the aborigines, especial attention being given to the tribes of the United States.

2. The United States National Museum has been conducting researches throughout the world, based chiefly on the material in the Museum, though partly on field study and collecting, special stress being laid on technology.

3. The Peabody Museum of American Archæology and Ethnology at Cambridge has devoted its energies to the investigation of the archæology of the United States, Mexico, and Central America.

4. The American Museum of Natural History, in New York, prosecutes work in ethnology, archæology, and physical anthropology, especially in North America, in Mexico, and in northeastern Asia.

5. The Field Columbian Museum, in Chicago, is making large collections and conducts ethnological and archæological researches in western-central North America.

6. The Free Museum of Science and Art, in Philadelphia, is bringing together collections from various parts of North America and subjecting them to critical study.

7. The Ohio State University maintains a department of archæology and a museum devoted mainly to local studies and collections.

8. The University of California is making investigations in archæology and ethnology, chiefly in California.

9. The National Museum of Mexico is accumulating a rich collection and conducting investigations in archæology and, to some extent, in the ethnology of the Republic.

10. Collections and investigations relating to the archæology of Canada are made, and a museum is maintained in Toronto in connection with the Department of Education.

11. The British Association for the Advancement of Science has carried on archæological researches in Canada since 1888.

Several institutions in South American countries are engaged in related work. The Archæological Institute of America, although mainly engaged in researches in classical and oriental archæology, has recently taken up America. Several American universities and colleges devote more or less attention to local or general archæology and ethnology. Investigations in archæology are pursued at the Harvard and Columbia Universities and at the University of Chicago.

American anthropology finds active patronage in several voluntary associations. The Section of Anthropology of the American Association for the Advancement of Science has long stood at the head of such organizations. The American Anthropological Association has recently been founded and already embraces in its membership all the leading anthropologists of the country. The Anthropological

Society of Washington and the American Ethnological Society of New York are doing excellent service, both local and general, in various branches of anthropology.

The greater part of research, with the exception of that of the Bureau of American Ethnology, that of the voluntary societies, and that of the universities, is characterized by its immediate relation to the material culture of the aboriginal tribes, and very little investigation is being carried on by these institutions that can not be illustrated by museum specimens. For this reason the anthropological work done in America is somewhat one sided, the physical branch especially remaining undeveloped.

It may be appropriately noted in this connection that the concentration of American anthropology upon the peoples of our continent has the effect that the general comparative points of view are in danger of being obscured by the excessive weight given to local phenomena. It is one of the greatest desiderata for the training of the young ethnologists that they should be given opportunity to become acquainted with the culture of Africa, Asia, and of the islands of the Pacific ocean.

The following suggestions are made with the view of strengthening what seem to be the weaker lines in American anthropology, thereby rounding out and perfecting the science as pursued in this country.

IV. Suggestions as to Researches in Anthropology by the Carnegie Institution.

(1) *Researches in Physical Anthropology.*—A most important field of research comprises the problems of physical anthropology. Facilities furnished by present institutions are entirely inadequate, and the investigation has at no point been systematically pursued. These vastly important problems can be successfully solved only in a well-equipped laboratory, the permanence of which for a considerable time is assured. One of the most important problems to be treated is the development of types of mankind from childhood to the adult stage as determined by heredity and environment. Up to this time it has never been possible to cultivate, systematically, this important field of investigation, for lack of means and men. It is therefore suggested to establish a central anthropometric and psychometric laboratory, which shall collect and discuss data on the development of physical types.

The treatment of the problems should be based on continued observations upon the same individuals from early childhood until maturity and old age. The characteristic physical and mental development of various races and types of men can be determined by this means. The collection and discussion of this material will throw light upon the laws of heredity, upon the development of types, and upon the conditions which favor and retard physical and mental development. The last named subject is one which previous results have shown to be of great practical importance.

(2) *Researches in Archæology.*—In America the anthropologist has to deal with various classes of aboriginal remains which illustrate the pre-Columbian history of the native peoples. Two somewhat distinct divisions of research are included. The first relates to the better known remains of cities, towns, dwelling sites, fixed works, and artifacts of many classes; the second to such of the earlier remains of man and his handiwork as bear definite relations to geological and artificially stratified formations, thus affording a basis for chronologic differentiations. Within the area of the United States the former division has received much attention, and numerous agencies are now engaged in its study, so that no additional work is recommended. In middle and South America much is still to be done, but your committee has only been able to find, and that at the last moment, one person properly qualified for this branch of research.

The second division of this subject may well receive attention from the Carnegie Institution. The phenomena are scattered and obscure, and agencies now in existence have not been able to enter systematically upon their study. The geological formations of both continents, ranging from Eocene to Recent, abound in varied records, but investigation has been, in the main, desultory and unscientific, and the isolated observations are today without adequate correlation.

If researches in this field be undertaken, the first step should be a compilation of all available data and a correlation of the results of previous investigations. The field work should begin preferably near home, extending later to favorable localities in various parts of the world. Examinations should extend to deposits in rock shelters, caves, and caverns, where men have lived and where horizons are so marked as to afford a basis for chronology. They should include various other ancient occupied sites, such as kitchenmiddens, shell-heaps, and earthworks, whose strata serve to indicate successive occupations. Researches in glacial formations where traces of man

occur are of the utmost importance, and outside of the glacial areas there are many sites that, properly studied, should yield most valuable results.

It is recommended that researches in this field be taken up, beginning with the exploration of rock shelters and caves in the eastern United States. The aid of competent geologists should be sought in selecting such as are of considerable antiquity, and in which the deposits are likely to afford definite indications of chronologic sequence.

(3) *Researches in Ethnology.*—The third field of research to which special attention is called is the systematic study of the fast vanishing tribes of American aborigines. As before indicated, it is receiving limited attention from various agencies which are, however, inadequate to the needs of the case. It is therefore suggested that the Carnegie Institution take up systematic studies in this field.

In different parts of America distinct forms of aboriginal culture have developed. The importance of carrying on investigations concerning these with the greatest energy and with as little delay as possible can not be too emphatically urged. In the area of the United States and Canada alone, approximately 350 languages are spoken. Of these languages, not more than 20 are tolerably well known, while of the rest we have nothing but brief vocabularies and unsatisfactory grammars. Each tribe that speaks a language of its own has also a culture of its own that should be investigated. A comparison of languages and cultures in detail is the only means of reconstructing the earliest history of the American race, and only the reconstruction of this history can give us the comparative material that should be furnished by our continent toward the study of the laws of development of human culture. Inside of the next ten years one third of the remaining 350 languages will have disappeared. In twenty five years it will be impossible to obtain of these languages more than vocabularies, and the culture and native ideas will have disappeared completely. These statements apply with almost equal force to the native peoples of Mexico, Central and South America. If this great body of the subject matter of human history is to be saved for the future, active researches should begin at once. Up to this time only a few tribes have been studied with any degree of thoroughness. Existing agencies are investigating throughout the following tribes:

Eskimo,	Sioux,
Tribes of the Northwest Coast,	Arapaho,

Salish,	Kiowa,
Northern California,	Pueblos,
Iroquois,	Shoshones.

Superficial or fragmentary work has been done among the following tribes:

Pawnee,	West Algonquin (except Arapaho),
Lower Mississippi,	Athapascan,
Muscogee,	Kutenay,
South California,	And other small tribes.

All this work is urgent. At the present time the various tribes of the Caddoan stock deserve special attention. They have never been studied scientifically, and today they are rapidly decreasing in numbers, the total population being about two thousand two hundred. To this stock belong the Pawnee, the Arikara, the Wichita, the Caddo, and the Kichai. Taken collectively they form the least known and most interesting large group of Indians in the United States. Apart from the intrinsic merits of these tribes as subjects of investigation, they occupy and have occupied a median position between the tribes of the Great Plains and those of the extreme south, and hence form a most interesting connecting link. All the tribes of the group have an extensive mythology and very elaborate ceremonies, with rituals which are chanted and which for their value in the study of primitive culture are unparalleled. Their material culture was comparatively high, and, among other things, they evolved two highly specialized forms of habitation, the Pawnee earth lodge and the Wichita grass lodge, in the construction of which most interesting ceremonies are performed. Because of the complexity and multiplicity of their ceremonials, and especially on account of the necessary expense involved, no investigator has hitherto devoted himself exclusively to this group. The expense of ordinary work of this nature is here more than doubled, because the many hundreds of songs must be recorded on a phonograph and transcribed in both words and music, and to secure this material requires an exceptional interpreter, for all these rites are secret, and the interpreter must not only be influential, but must be of the priesthood. Again, all the rituals are regarded as personal property, and may not be transferred except on payment of goods or money. In addition to the many ceremonies with elaborate rites, the Pawnee have also sacred and secret bundles, which are only opened ceremonially when an

extended ritual is sung. There are over four hundred and fifty songs in a single bundle. One of the bundles is dedicated to the morning star, and formerly was opened only when the rite of human sacrifice was performed.

The object of the suggested research, then, is to complete a study of the culture of the different tribes of the Caddoan stock, special attention being given to (1) the rituals of the Pawnee ceremonies and sacred bundles, and (2) the mythology and linguistics of all four tribes.

Respectfully submitted.

W. H. HOLMES, *Chairman*,
FRANZ BOAS,
GEORGE A. DORSEY,
Committee.

OCTOBER 16, 1902.

REPORT OF ADVISORY COMMITTEE ON BIBLIOGRAPHY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Committee have thought that the Trustees might wish to have the consideration of particular projects prefaced by a survey of the general field in each department of science with which it may propose to deal. In this belief the following statement is presented:

In the case of Bibliography a concise statement is impracticable, not merely because the field is vast and indefinite (embracing as it does not one science, but a consideration of the literature of *all* the sciences), but because the work already done, or in progress, or projected, is so considerable and includes undertakings so numerous and so diverse in scope and method that the precise area covered by them can not briefly be described with precision.

A mere catalogue of existing bibliographies would, it is estimated, comprise over 25,000 entries.*

The appended memorandum,† drafted by Dr. J. D. Thompson and other bibliographers of the staff of the Library of Congress, indicates certain of the more notable achievements or projects to date. It is appended, not as a complete statement of the work already done, nor as a demonstration of the work which remains to be done, but as a suggestion of the multitude and diversity of the undertakings which must be examined before the opportunities remaining to the Carnegie Institution can be fully defined.

A brief reference to the memorandum will indicate the extraordinary activity that has existed and still exists in bibliographic research and publication. There have been bibliographies covering certain departments of literature, or certain periods, or certain geographical areas; the literature of the past or the literature in process of issue; and even attempts (of which one is still in progress) to cover all the existing literatnre on all subjects. The work has been done in part as a commercial venture, in part by societies, institutions, or governments, as a contribution to knowledge. To bibliographies, properly so called, which attempt to exhibit all the

* Margerie's "Catalogue des bibliographies géologiques" alone contains nearly 4,000 entries.

† Not here printed.

literature upon a given subject, or within a certain area or period, there must be added the catalogues and topical lists issued by libraries of material in their own collections, where the collection has been developed with a view to relative completeness. Nor can there be omitted from consideration trade catalogues, reviews in current journals, and selected lists of authorities appended to treatises; for any proposal for a grant by the Carnegie Institution assumes that the investigator is in present need of information as to the literature of his subject not now conveniently, precisely, or adequately accessible to him. If, for example, it be proposed for the Institution that it shall undertake a comprehensive bibliography, the work of the Institute at Brussels must be reckoned with; if a national bibliography for the United States, Sabin, the American Catalogue, the publications of the American Library Association, the Card Indexes of the Library of Congress, and other undertakings which in the aggregate are likely to cover, even though unevenly, this area; if the literature of the natural and physical sciences, the Royal Society Index, the International Catalogue of Scientific Literature, the publications of the Concilium Bibliographicum at Zurich, and the various Centralblätter and Jahresberichte, etc., etc.

On the other hand, the existing bibliographies vary greatly in form, in method, in accuracy, in completeness, and in accessibility; so that the mere existence of a bibliography dealing with a certain branch of science, or a certain period or area, is by no means conclusive against a proposal for further work within the same field—or in continuation—for the subject matter continuing, the bibliographic record of it is never ended. A bibliography thorough within its field may be insufficient because it includes entries by author only, while the investigator requires a classification by subject; or it may be on cards, while his convenience requires a publication in book form; or, if of current literature, its issue may be so tardy as to defeat its utility; or, having all merits to his need, it may have failed, or be in peril of failing, as a commercial venture and require and justify a grant in its aid.

Your committee deems it futile in this report to define a resultant appropriate field of activity for the Carnegie Institution. It contents itself with responding to the particular proposals already submitted, which it has endeavored to test by a consideration of the existing facilities, and to recommend for immediate action only two, the first of which (the Index Medicus) has already demonstrated its utility and necessity, and the second of which (the Handbook to

Learned Societies) is not so much a bibliography as a necessary preliminary to *any* thorough bibliographic work involving the literature of science.

Respectfully submitted.

HERBERT PUTNAM, *Chairman*,
CYRUS ADLER,
J. S. BILLINGS,
Committee.

OCTOBER 20, 1902.

Supplementary Suggestions by the Chairman of the Committee, January 5, 1903.

The field of bibliography is all existing literature, with continuations.

From time to time there have been projects of a universal bibliography. There is one such project now under way. The International Institute at Brussels is attempting a universal catalogue, by author and by subject, or rather by class, the classes being based on the decimal system. The entries are, for the most part, composed of clippings from catalogues. They are thus made at second hand. They lack the bibliographic value which exists in an entry made direct from the book itself. They are on cards, and thus lack the utility possible in a catalogue, copies of which are multiplied in book form for distribution. Granting, however, the possibility of a universal bibliography, no member of your committee would, I think, have recommended it for the consideration of the Carnegie Institution. The field is too vast, the expense too great, the utility of the results, in the only form which they could be secured, too doubtful.

The field of bibliography may be *divided* in three ways: (*a*) by territory, (*b*) by subject, (*c*) by period.

(*a*) In a division by territory the area for the Carnegie Institution would naturally be the United States. Attempts to cover the literature of this country are indicated under National Bibliographies, United States, on page 5 of the Appendix to the Committee's Report. The completion of Sabin, which now comes down to the letter S, is highly desirable, but it is likely to be undertaken as a commercial venture by the successors of the firm which instituted the work. The printed cards of the Library of Congress will in the course of

the next five years embrace the largest single collection of American publications, in the National Library, which is attempting to secure every American imprint of possible concern to research. The printed cards of Harvard University, the Boston Public, and the New York Public libraries (copies of which will be on deposit at the National Library) will in large measure supplement the record based upon its own collections.

In view of these and other sources of information open to the serious investigator, the Carnegie Institution could not, I think, be asked to undertake a national bibliography for the United States.

(*b*) Division by subject: The area appropriate to the Carnegie Institution will, of course, be that with which the research may be concerned which it is its intention to promote. This is, Science. How far this term extends has not, I believe, as yet been defined. The assumption has been general that preference would be given to the natural and physical sciences. In these the material of most concern to the investigator consists (1) of the current publications, and (2) of the publications of the preceding ten years, or at least of the preceding quarter of a century.

Current publications are to be covered by the International Catalogue of Scientific Literature. This catalogue will be based upon contributions from twenty-seven regional bureaus. Were not the Smithsonian already the bureau for the United States, the Carnegie Institution might well become so. No contribution by it to bibliography in aid of research could be more appropriate or more useful than this: the territory, the United States; the subject matter, the natural and physical sciences; the period, the present and the future.

Certain sciences are not to be included in the scope of the International Catalogue. These are the *historical*, the *philosophical*, and the *philological* sciences. All applied science is omitted. The current literature of applied science, engineering, etc., is fairly represented in the *Repertorium der technischen Journal Litteratur* issued by the Patent Office in Berlin, and by less comprehensive indexes in English. The current literature of history, of philosophy, and of philology is not, however, satisfactorily cared for by any existing comprehensive bibliographies. An index to the current literature of these sciences, if it could be undertaken by the Carnegie Institution, would be a most important and practical contribution to research. It would complement the International Catalogue. It might presumably be based upon the work of regional bureaus, precisely as is the International Catalogue, the Carnegie Institution assuming to it the

initiative and relation which the Royal Society has assumed to the latter enterprise.

(*c*) As to period: I have already indicated my opinion that for the Carnegie Institution, created to promote research, the most serviceable contribution in bibliography will be that which exhibits the recent and current literature rather than that which is retrospective. The investigator who is to advance the boundaries of knowledge will not, except as he is a bibliographer or historian of his subject, have much occasion for retrospect. In so far as he has occasion for such, he will require not a mere list of titles, but the actual books themselves. For these he must have recourse to a particular library or libraries. It is the duty of those libraries, through their catalogues, to furnish him with a statement of their contents. In the field covered by the International Catalogue, the Royal Society's "Catalogue of Scientific Papers," already covering the period 1800–1883, and proposing to cover also 1884–1900, is so nearly comprehensive as to render parallel attempts extravagant; just as in the field of medicine, for which the Index Medicus will cover the current literature, the Catalogue of the Surgeon General's Office Library forms for all practical purposes a comprehensive statement of the existing literature.

In considering undertakings more special, within a narrower field or a particular department of literature, the following considerations should apply:

I. In any subject in which there is active research, accompanied by a continuing literary record, a bibliography to be serviceable must also be continuing. A grant of a given amount will therefore, as a rule, be more effective if applied to a continuing bibliography within a narrow field than if exhausted upon a (periodically) limited bibliography within a larger field.

II. A bibliography differs from a selected list of titles, on the one hand, and from a catalogue of a particular collection, on the other. It attempts to be a complete exhibit of the literature of the subject. Such completeness exists in no single place or institution. A bibliography compiled at second hand can, however, be of but little authority. A bibliography which consists merely of brief titles, without explanation or analysis or an attempt to locate the material, can be of but meager utility. The preparation of a serviceable bibliography requires (1) direct use of the completest existing collections of material; (2) the most efficient bibliographic tools; (3) expert bibliographers, not merely specialists in the subject matter; (4) promptness and frequency of issue; (5) a form of publication which

will permit of distribution; (6) a form which will admit of the possibility of use by an individual investigator without great expense for accommodation and arrangement.

III. Duplication of bibliographic work is to be avoided. Coöperation is to be sought.

IV. The above considerations render inadvisable aid by the Institution to an undertaking which is isolated or fragmentary, which is not likely to be continuing nor practically exclusive within its field. It must, for instance, render inexpedient grants for the compilation or publication of a bibliography appended to a mere monograph on a particular subject, published as a commercial venture. The purpose of such an appendix can, as a rule, be better served by a selected list with discriminating notes than by a bibliography. Its circulation will be limited to that of the main work and controlled by commercial considerations, which are not controlling considerations with the Institution.

V. With the possibility of an undertaking which shall cover a large subject matter of concern to research, the Institution would, I think, be unwise to parcel its funds for bibliography by a number of small grants in aid of bibliographies of special subjects.

VI. *An aid which the Institution may render, of prime importance to all science, including the science of bibliography itself, would be to coördinate and correlate existing bibliographic sources, undertakings, and projects, to acquire and disseminate information which will exhibit the character of each, and the relations between them, and will prevent unnecessary duplication of effort and expenditure.*

I understand that the funds of the Institution available for bibliographic work during the coming year have been pledged in grants already made. I refrain therefore from expanding the above suggestions. I submit them now in explanation of the considerations which I should have in view in reporting upon any particular applications referred to me for recommendation.

NOTE. *Sources for Research in History and Sociology.*—These are scattered in institutions and archive offices here and abroad. To locate them with precision, to secure exact descriptions of them, and information as to the means and methods of access to them, and to publish these data for the information of investigators would be an obvious and important service to research. To secure transcripts of them and to concentrate these at some point most convenient to the

main body of investigators would advance the service into one of the highest utility.

Such an undertaking has been suggested as appropriate for the Carnegie Institution, and was mentioned in the deliberations of the Advisory Committee on Bibliography, but was deemed more appropriate for consideration and recommendation by the Advisory Committees on Historical and Economic Research. A proposal for an investigation into the sources for historical research at Washington has already been acted upon. Should similar investigations be undertaken of the sources in other places and abroad, accompanied by transcripts of important material, I should be glad, as Librarian of Congress, to submit some suggestions as to possible contributions to them by the Library of Congress which may result in a broader scope, a greater efficiency, and less expense to the funds of the Institution.

Very respectfully,

HERBERT PUTNAM.

REPORT OF ADVISORY COMMITTEE ON ENGINEERING

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: In response to the suggestion that the Advisory Committee submit such suggestions as are deemed helpful in determining the policy, methods, and acts of the Carnegie Institution, your Advisory Committee on Engineering herewith respectfully offers the following:

Preliminary.—The Founder has stated his intent, in the organization of this Institution, in simple and very definite terms, thus indicating clearly his primary purpose to be the promotion of the highest welfare of the people of the country, through scientific research, and incidentally to aid individuals who have been successful in that vocation, seeking mainly to help those who have most effectively helped themselves in the prosecution of that work.

The chief purpose of the Founder being, if possible, to secure to the United States of America leadership in the domain of discovery and "the utilization of new forces for the benefit of man," as stated in the deed of gift, it follows that such work in research as promotes the industrial arts and improves the system of production of the country is most important, in the views of the Founder. Scientific methods and scientific work in the field of engineering, that profession which devotes itself to the advancement of these arts, is directly in the line of the Founder's ideal.

The general scheme of the Carnegie Institution seems to contemplate something in the nature of a university, but devoted to research instead of education. A university as we commonly understand it is the head of an educational system combining the work of a museum with that of a school. The Founder of the Carnegie Institution seems to have recognized that there is another function not included in either of these. He has designed this Institution to be an investigator; in determining its functions and duties this should be remembered. The Institution proposes to supplement the work already done by the universities, but to supplement it on lines of its own. In any work undertaken by the Carnegie Institution its own identity should be maintained. Coöperation with others should only be directed to the prevention of duplication of work. The Institution should never subordinate itself to any other institution.

The aims of the Institution, as declared by its Founder in the deed creating the same, are:

1. To promote original research, paying great attention thereto as one of the most important of all departments.

2. To discover the exceptional man in every department of study whenever and wherever found, inside or outside of schools, and enable him to make the work for which he seems specially designed his life work.

3. To increase facilities for higher education.

4. To increase the efficiency of the universities and other institutions of learning throughout the country, by utilizing and adding to their existing facilities and aiding teachers in the various institutions for experimental and other work, in these institutions as far as advisable.

5. To enable such students as may find Washington the best point for their special studies, to enjoy the advantages of the Museums, Libraries, Laboratories, Observatories, Meteorological, Piscicultural, and Forestry Schools, and kindred institutions of the several departments of the Government.

6. To insure the prompt publication and distribution of the results of scientific investigation, a field considered highly important.

These would seem to divide themselves into three groups. The first is the organization of the Institution itself. The second is analogous to the systems of fellowships established in a university. The third is publication.

The aims designated by the Founder as Nos. 1, 3, and 4 will come under the immediate direction of the Institution. The aims designated as Nos. 2 and 5 will come under a general supervision of the nature of fellowships. No. 6 will constitute the department of publication.

Institution.—The Institution should be conducted by a group of men, specialists in their own departments, capable of directing investigation and of recognizing the value of work done elsewhere. There should be among them that proper *esprit de corps* which belongs to any loyal faculty. Their primary duties are indicated by No. 1. The methods by which they will promote No. 3, "to increase facilities for higher education," must be developed as the Institution progresses. It would seem that at first little should be done in this way beyond putting the results of the researches which they encourage in such shape that they will be available for all who are seeking higher education. The trust deed apparently contemplates financial assist-

ance to other institutions, but this should be given only on the principle that the teachers and the faculties which are so helped should act with and be employed by the Institution. Even with this condition, the extension of financial aid to such institutions should be very carefully guarded.

Fellowships.— Two classes of fellowships appear to be contemplated. The first is for life, and its character is indicated in No. 2. The man who wins one of these awards would receive an income on which he could live comfortably, and this income would continue so long as he should devote himself to the work selected and should perform it for the Carnegie Institution. The second class, provided by No. 5, is of more limited period, and would consist of resident fellowships in Washington. To these it would probably be expedient to add a traveling class, which might enable students to pursue their specialties in other cities and countries.

Two conditions should be recognized in granting these fellowships: *First*, they should be given only to candidates of established ability; *second*, the holders must recognize that their work belongs to the Carnegie Institution, through which the results of it will be given to the world. The awards will be one of the most difficult tasks which the Institution has to perform.

Publication.—The duty of the department of publication will be to publish promptly the results of the work of the Institution, this including everything that may be done in other institutions which are temporarily acting in conjunction with it. Promptness will be a prime necessity, as the value of the results of new investigations decreases rapidly with time. It will be expedient to issue two series of publications—the first in the form of advance sheets for immediate distribution among people known to be engaged in kindred studies, the second final memoirs in corrected shape.

As there is nothing in which time and money can be wasted more completely than in poorly conducted investigations, it will be important to classify different departments and subdivisions of departments which are recognized as proper fields of work for the Institution. Until such classification is made, it is very doubtful whether any specific grants should be given; furthermore, until the organization of the Institution is effected it is doubtful whether anything more should be done than to pass on the general expediency of propositions submitted, leaving final determinations for the future.

There is danger that, under the provision which permits subsidies, funds may be deflected from this Institution to purposes for

which they were not designed. All appropriations should be made for the purpose of promoting original research and not for education as it is commonly understood.

This committee is an Advisory Committee on Engineering. The greater part of an engineer's work is actual designing and execution. Neither of these is properly a subject for consideration or aid by the Carnegie Institution. Investigation and research which will aid engineers in the preparation and execution of their designs are proper functions of the Institution. Sanitary engineering must be based very largely on biological and other studies which properly come under other committees. The same may be said of mining engineering, the researches for which are largely geological and metallurgical. The specific lines of investigation and research which are left to this committee relate to the general subjects of energy and material.

The fields of scientific research in engineering are open to almost every department of science and include as their main divisions:

1. The physical characteristics of materials of construction, as cohesion, ductility, elastic limits, moduli of elasticity, their temperatures of fusion, volatilization, ignition, and decomposition.

2. Chemical composition, conditions of analysis and synthesis, methods of reduction of metals, of purification, of perfection in attainment of desired properties, etc.

3. Studies of methods and processes of production of the materials of engineering, and of manufacture, involving the employment of every art of the chemist and of the physicist.

4. Investigations of the work of the engineer in applied energetics and thermodynamics, including the character and value of fuel as a source of energy, its combustion, the transfer and storage of resultant energy in the working fluid employed in the engine, the nature and method of waste in the production and transfer of that energy, the process of energy—transformation in the heat engine—and similar studies in electric, pneumatic, and hydraulic energy utilization.

5. Research relating to the production of waste energy produced by friction.

6. Investigations of the class of those of Langley and Very on the relative efficiency of light producers, as of the fire fly and the candle or the electric light, determining the method of production and utilization of energy in the form of light.

7. Production of electricity from the potential energy of fuels without indirect transformation wastes. The process sought to be

revealed is that of a direct transformation of the potential energy of oxidizable substances into electric energy.

The field of research is too extensive to be described minutely, and the work in progress too varied and widely distributed to be indicated except in the most general way. For present purposes, however, it may be said that every important institution of higher learning, every professional school of engineering, a large and increasing number of business concerns, and several departments of government—general, state, and even sometimes municipal—are more or less well outfitted for such work and are engaged more or less extensively in its prosecution. The various technical associations and engineering societies are encouraging and stimulating research by affording opportunity to ascertain the immediately pressing questions to be thus solved, and by giving opportunity for the presentation and discussion with experts of the results obtained.

Even in the direction of finding men of genius, all these institutions and organizations are doing something and contributing, in some degree, toward providing time and opportunity for the display of scientific learning and of genius in research, discovery, and invention. But neither the universities nor the governments, even of the most advanced nations, are as yet finding ways to systematically identify the great investigators and men of genius in this work and provide them their opportunities.

Every school or college of engineering today includes in its curriculum far more extended courses of study and of laboratory work in chemistry, physics, and engineering science than is or ever was provided in any other department of instruction. Research in engineering is thus distinctly recognized as a fundamental element of progress. It is for these reasons that the work of the Carnegie Institution must probably find place very largely in connection with engineering and applied sciences which are its special basis. Both the attractions and the opportunities are large in this field, perhaps larger than in any other known department of human knowledge.

The fellowships to be instituted under the provisions of the deed of trust and all similar aids to individuals must evidently be carefully guarded against degeneration into the form of fellowships sustained by our institutions of learning, where the purpose is to aid education. Here the aim is research, and every appointee of the Institution must obviously be competent to make full return in fruits of scientific research for the assistance thus rendered. This neces-

sity will restrict these appointments to the few who have already exhibited genius and productive power and established habits of work in investigation. It will be particularly necessary to use caution in making appointments in aid of applicants without ample and well established records.

In pursuance of the policy of aiding those who help themselves and of making the income of the Institution go as far as is in any way practicable, as indicated in various ways by the Founder, it would evidently seem unwise to employ its income in the purchase of property, other than that contemplated in Washington, in the equipment of individual enterprises, or perhaps in any other way, generally, than the distribution of that income in small sums which may do most work by supplementing important efforts where a little additional aid may make complete and perfect a great work. Numerous opportunities will be found to secure comparatively important returns at little cost, and the innumerable applications for aid in all sorts of wise and foolish ways which come before the committees give evidence that only by carefully and safely providing for the most important objects in view can serious waste of funds be avoided.

The relative importance of a department of research which directly promotes the progress of the race industrially is so obvious and so enormous that your committee feel justified in submitting the proposition that the largest practicable support should be extended to all work bearing upon its extension and maintenance.

The first step to be taken in systematically preparing the way for the great work to be carried on by the Institution and insuring the highest possible efficiency of the operation of the latter would seem to be the systematic gathering of information relating to men, equipment, and facilities already existing, ascertaining where a deficiency exists, where ample provision is already made, what are the deficiencies and how they may be best remedied, what is the best way to make immediate and productive use of those which are in one or more lines substantially complete and satisfactory.

In this work it is to be presumed that the advisory committees may effectively aid. Each in its special department may find the men, discover the evidence of genius. By the issue of circulars of inquiry it will be easily possible to ascertain substantially what apparatus and facilities every institution of learning, every industrial enterprise, and every contemporary investigator has under control in the field assigned; thus, as a supplementary result, learning just what

in each case may or should be done by the Institution or others to make the inventory complete for purposes of important research.

These methods have all along been obviously proper introductories to the work, but this committee has not felt authorized to enter upon their promotion until specifically authorized so to do by the Trustees of the Institution as part of a general and well considered scheme approved by them. The committee has confined itself thus far to the consideration of matters referred to them by the Executive Committee.

Recommendations.—The specific *recommendations of the committee* are, following the scheme of the Founder:

1. That promotion of original research, rather than education as commonly understood, be made the primary object of the Institution.

2. That this object be promoted by—

(*a*) The discovery of the exceptional man and assistance to him to enable him to make the work for which he seems specially designed his life work.

(*b*) By increasing the facilities for higher education by ascertaining just where those facilities are most likely to prove fruitful of good to the country and to the world, by discovering what facilities already exist, and, finally, the best way of supplementing well established and safely organized equipments for advanced and professional education and scientific work.

(*c*) By utilizing and adding to existing facilities of the universities and other institutions of learning throughout the country and aiding their teachers in research.

3. That special aid be extended to fellows and other individuals for stated work; these fellows to be temporary, permanent, traveling, etc., in order that all existing sources o knowledge may be utilized to promote research.

4. That a permanent Carnegie Institution organization be established, consisting of—

(*a*) Committees, general and advisory.

(*b*) A faculty of a rather limited number of individuals, who are specialists in the various departments, with a staff capable of conducting the routine business of the Institution.

5. That few, if any, Carnegie laboratories, workshops, or schools be established as parts of same; nor should those in existence be taken over, thus saddling the Institution with management duties in widely separated places and placing it in competition with existing institutions.

6. That special Carnegie Institution publications shall promptly give to the world the results of the Institution's work—that is, the work of its faculty, fellows, and aided individuals and institutions. The circulation of these publications should be wide, to reach all deserving persons interested, and at a nominal price. It is suggested that later a plan can be devised to supply promptly and thoroughly edited science abstracts, as filling a most important need.

Respectfully submitted,

R. H. THURSTON, *Chairman*,
W. H. BURR,
GEORGE GIBBS,
GEO. S. MORISON,
CHAS. P. STEINMETZ,
Committee.

NEW YORK CITY, *October 24, 1902*.

REPORT OF ADVISORY COMMITTEE ON PSYCHOLOGY

CONTENTS.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN : In preparing this report certain general considerations occurred to the mind of your committee :

I. General Considerations.

Psychology stands over against the natural and physical sciences, as underlying what may be grouped together as the sciences of man. This latter group of sciences includes anthropology broadly defined, history, and the mental sciences proper in their association with the moral and social sciences. It has become more and more the recognized view that exact work in psychology must precede and furnish the foundations of the scientific structure in each of these branches of knowledge. Accordingly, if we should say that physics and chemistry were the fundamental physical sciences, and that biology, used to include both botany and zoölogy, was the fundamental science of life, psychology would hold a corresponding place in relation to the entire group of what we have called above the sciences of man. Too great importance, accordingly, cannot be attached to the treatment of this subject in any scheme of the sciences adopted by the Carnegie Institution, which proposes to encourage and provide for research in science generally. The fact that the physical and natural sciences have already had a richer development should not prejudice the claim of psychology to the fullest recognition ; on the contrary, such a fact only gives increased emphasis to the needs of this department, provided the provision made for it can be both definite and fruitful.

The history of this science in the last twenty years has been one of remarkable progress. Psychology has become a positive science,

and the actual results of the application of the experimental method are, we venture to think, greater, relatively speaking, than those of any other science in the corresponding period. Sufficient advance has been made to justify the establishment, in the great universities, of laboratories and other material aids to psychological research. The literature of the subject, as listed in *The Psychological Index*, reaches for 1901 an astonishingly large number of titles, and two great reviews are regularly published in this country alone, one of them also finding it necessary to issue large supplements for the printing of extensive pieces of research.

Certain more general features of contemporary psychological work, when viewed with reference to such a grouping of the sciences as that suggested above, may be pointed out. In the first place, the development of psychology is necessary for the sound solution of most important problems in the physical and natural sciences. In physics, for example, matters of direct psychological import come up for treatment, such, for example, as the question of the personal equation in astronomy, the question of the normal function of the senses in physical observation, the question of the extent and psychological justification of the various methods of research—all problems in which, as history shows, final solutions have waited upon the results of psychological criticism and research. In the biological sciences the same is true, but to a greater extent. The development of genetic psychology in recent years has been perhaps the most important modifying influence in general biological theories. The problem of evolution is now as much psychological as biological, and the biologists themselves are prepared to ask the coöperation of expert psychologists in their most difficult undertakings. Apart from this relation of psychological investigation to that in the physical and natural sciences, the direct value of psychology in connection with all the sciences of man remains over—a fact which in itself is sufficient justification for the most liberal endowment of psychological research.

The principal function, as we conceive it, of such a new establishment as the Carnegie Institution in relation to psychology should be that of *unification*—the function which is of the first importance in reference to the relationships of all the sciences. The development of psychology, rapid as it has been, has been along distinct lines. We have today no less than five general undertakings, all yielding fruitful results and each pursued by more or less independent methods, namely: 1st, Laboratory Psychology (including

both Experimental and Physiological Psychology); 2d, Genetic or Comparative Psychology (including Zoölogical and Anthropological Psychology); 3d, Social, with its important branch, Educational Psychology; 4th, Statistical Psychology (including questions of mental variation, heredity, types of mind, etc.); and, 5th, Pathological and Abnormal Psychology—the investigation of diseased and abnormal minds. The course of development in these somewhat distinct and separate lines has been so self controlled that proper relationships do not exist among these departments. It is now of the most extreme importance—and it is indicated above as one of the most evident functions of the Carnegie Institution—that there should be an agency for the better unification of researches in these different fields. This, and with it the direct encouragement of research in each of the great subjects mentioned, with their subordinate subdivisions, constitute, in the opinion of your Committee, the main topics for recommendation in this report.

The presentation now made of the present condition of psychological inquiry may be supported by reference to a recent undertaking in Paris. I refer to the foundation in 1900, in connection with the International Congress of Psychology, of what has been named the General Psychological Institute. This association, formed under the patronage of an international committee, upon which your present reporter was asked to serve, has for its explicit objects: *First, the advancement of psychological science*; and, *second, the unification of the branches of psychological work*—the two objects which the present report is also emphasizing. The importance of the function of such a general institution in unifying the results of science is seen, or may be seen, in the following quotation from a recent bulletin issued by this French Psychological Institute: "The branches of psychological science [says this report] appear to be pursued in too great independence of one another. Properly speaking, they should be so closely connected that it would be impossible to make a profound study of the facts of any one of them without thorough knowledge of the body of results from the others. How, for example, can we study the psychology of children without knowing the work that has been done on such questions as that of the general psychology of suggestion? Or how study the mental condition of the alcoholic inebriate without knowing all the results of research which bear upon the psychic effects of drugs? In short, *solidarity, in this science, has become a great need.* The tendency to form international congresses, and, in particular, the work of the

psychological congress, clearly shows the need and the utility of this solidarity. Partial groups of workers can be but temporary. There is need, therefore, for a permanent center, where the different branches of psychological science can be brought into continual and helpful contact. Then only will all the scattered efforts made in different places be made to converge upon special, broader points of study, with the result that light will be thrown upon them all by this effort at synthesis. This is precisely the controlling idea in the foundation of the General Psychological Institute."

This report is issued by a board of expert psychologists. It may be added, however, that this French Institute depends upon public subscriptions and dues of membership, and has so far been able to do little beyond the issue of programs and suggestions to single workers.

In the same connection the present reporter may also cite the policy of the recently issued *Dictionary of Philosophy and Psychology* of the Macmillan Company—an international coöperative undertaking of which he has acted as editor. The policy of this work, devoted to the mental and moral sciences and to the criticism and theory of science in general, is shown in the fact that psychology and psychological research have been made the center of the entire undertaking; and, furthermore, it appears that in this work it has been thought necessary to include the main conceptions of both physical and natural science—so intimate has become the relation of psychology to these other disciplines. The following quotation from the general preface to that work may serve to show its point of view. It reads: "Psychology is the half way house between biology, with the whole range of the objective sciences, on the one hand, and the moral sciences, with philosophy, on the other hand. The claim to this place made by psychology today is no more plain than is the proof of it, which the results in this department of research make good. The rise of experimental and physiological psychology has caused the science to bulk large towards the empirical disciplines as it always has towards the speculative; and the inroads made by psychological analysis and investigation into the domains where the speculative methods of inquiry were once exclusively in vogue, render permanent and definite the relation of that side as well. In biology, in sociology, in anthropology, in ethics, in economics, in law, even in physics, the demand is for sound psychology; and the criticism that is making itself felt is psychological criticism. * * * It will be found, therefore, that

it is upon the psychology of this work that most of its lines converge."

Again, in the following quotation the emphasis laid upon science by the editors of that work is brought out: "It is one of the safest sayings of philosophy at the close of the outgoing century that whatever we may become to end with, we must be naturalists to begin with—men furnished with the breastplate of natural knowledge. We must know the methods as well as the results of science. We must know the limitations of experiment, the theory of probability, the scientific modes of weighing evidence and of treating cases. Lack of these things is the weakness of many a contemporary writer on philosophy. Such a one criticises a science which he does not understand, and fails to see the significance of the inroads science is making into the territory which has so long seemed to be exempt. Note the application of biological principles, in however modified form, to psychological facts, the treatment of moral phenomena by statistical methods; and these things are but examples. These topics are becoming of special importance to the psychologist, the moralist, and the student of life."

Two general utilities are therefore pointed out by your committee as most important, namely: *First, the encouragement of definite scientific research* in psychology; and, *second, the unification, in what the French call solidarity, of the different departments of psychology.*

In the opinion of your Committee, both of these objects can be best subserved *by the establishment at Washington of a department of psychology in immediate connection with any other scientific establishments which the Carnegie Institution may found.* The provision for this science will then rank with that of certain other fundamental sciences in the appropriations of the Institution.

In such an establishment psychology should properly have two units from the entire number of units of appropriation in the scheme of the sciences; that is, if ten divisions of science be recognized as having claim upon the income of the Institution, it is the deliberately formed judgment of your Committee that psychology should be assigned two-tenths of the entire income. This, however, does not mean that researches which are only of psychological value would be so largely entered upon, for one-third or one-half of the work undertaken would be in joint control of psychology with some other science or sciences. This will appear from a concise statement of the principal problems of the several departments already mentioned.

II. Most Important Practical Undertakings.

Making the foregoing the basis of our more detailed suggestions, the two great divisions of appropriation may be these: (1) The support of a central institution with the several departments enumerated below; and (2) the establishment of a fund for grants and subsidies of various sorts for the direct encouragement of psychology throughout the country.

These two objects can, in the opinion of your Committee, be well combined in a way which will at once stimulate the psychological work of all the universities and, at the same time, supplement and further them.

It is assumed without discussion that the Institution is prepared to undertake grants for special researches. Your Committee is of opinion that a sum of from $5,000 to $10,000 can be profitably employed at present for such grants in psychology. This sum, however, it is evident, should be extremely flexible.

To enter into some details of recommendation as to the constitution of the department of psychology, your committee suggests in outline the following scheme:

First. A department for *Genetic*, including *Zoölogical* and *Anthropological Psychology*. In the classification which we are now presenting, the principle of which is no less economy than utility, two bureaus are here included—that for what is generally called *Comparative* or *Zoölogical Psychology*, and that for *Anthropological Psychology*.

The former of these aims to carry the investigation of the mind or consciousness into all of its manifestations in the animal world. It recognizes the great doctrine of evolution and its debt to the interpretation of the series of animal minds in terms of their genetic descent. We thus have the problem of *mental morphology* as it has been called by a prominent biologist—a problem which is as wide in its reach and as important in its solution as the great problem of morphology is to the biologist, since the rise of the modern Darwinian theory. Important beginnings have been made in the investigation of the animal mind; but the hindrances which have presented themselves to individuals and institutions in this research have been almost insurmountable, seeing that such investigation requires the keeping of typical animals of various habit, size, food, and care, and the breeding of these animals for considerable periods, in order that observations may be systematically carried out. Furthermore,

the observations themselves require the constant presence of trained observers, together with the carrying out of systematic and exact measurements. All this is an undertaking of such complexity and magnitude that nothing but a central establishment, where the conditions may be made constant and the observers free from other undertakings, would be adequate. This should be undertaken as part of the project, if such be carried out, of an experimental farm established for the investigation of biological and genetic problems. Failing that, however, a psychological station of the sort mentioned might well be established, at very moderate cost, in connection with one of the larger zoölogical parks of the country, to begin certain well planned researches. Washington presents facilities of this sort, and your Committee urgently recommends that this project be at once entered upon. By doing this the Carnegie Institution would be absolutely the first agency in the field for accomplishing work for which the time is ripe, and for which other agencies are strenuously exerting themselves in a partial way under their peculiar limitations. The coöperation of Psychology and Zoölogy in this matter is most desirable.

The second department of research under this general head of Genetic Psychology is that known as *Anthropological Psychology*. As the term indicates, this department investigates the psychological processes—in short, the minds—of races and peoples at every stage of culture. The urgency of the undertaking from the anthropological side has been recognized and, in many instances, met by private provision in recent years. It only needs stating that many savage peoples are rapidly disappearing from the earth, carrying with them all that is living of their customs, institutions, superstitions, religions, and mental parts in general. This only needs to be stated to convince the intelligent sympathizer with science that it is a principal duty of the modern world to collect facts in chosen regions while these facts still remain to be collected. Hitherto, however, the anthropologists have worked largely without the coöperation of psychologists. Despite the best efforts of anthropologists and their full sympathy with the psychological problems, their expeditions have not been properly manned for the carrying out of mental researches. Such work may be illustrated by the English expedition sent out by Cambridge University to the Torres Straits, with which a psychologist was sent having a fairly good equipment. The published results fully justified expectations. Psychological work was also attempted in connection with the recent

Jesup expedition sent out from New York. It is an undertaking of first rate importance which falls as an opportunity to the Carnegie Institution—a task which, like those already mentioned, will remain largely undone unless such a central institution undertakes to do it. The principal appropriation in this field would be to anthropology, but a trained expert and his adequate equipment would belong to psychology.

Second. A second bureau is recommended, under the terms *Educational and Social Psychology.* Educational psychology has both its theoretical and practical side. Theoretically it aims to formulate the laws of operation and function of the mind at different periods of growth, and with the recognition of different types of temperament and individuality. It thus aims to supply that knowledge of the growing individual which is requisite for his proper education and training from earliest childhood to maturity. The lack of such a body of knowledge based upon actual tests, measurements, and statistical observations, has been the great obstacle to the progress of educational science. On the practical side, such a department addresses itself to the use of such knowledge of the mind in arranging and controlling the actual work of the schools. The classification of school children, the differences of the sexes, their relative maturity at different periods, the proper distribution of time to different subjects, the adjustment of the body in periods of mental application, the laws of fatigue and recovery, the testing of the senses, the hygiene of the mind in the close social relationships in which children and youth are thrown, the systematic carrying out of tests and measurements upon college students—all these are of the important practical matters which affect the education of our youth. These matters can not, in the opinion of your Committee, be undertaken in a single school or university, nor upon a single type of individuals. They require large numbers of observations, repeated from year to year, the most careful devising of experiments to reach the mind without interferiug with it, and other equally broad arrangements which can only be carried out by a central institution provided with adequate facilities. Washington suggests itself as possibly unsurpassed in the material and opportunities which it would present for the prosecution of educational psychology. We suggest that this work should have the coöperation of the Government Bureau of Education, and it should be expected to be of material aid in the work of that Bureau.

The Social Bureau in this department, it is easy to see, should be

closely affiliated with the Educational. It should deal with a large range of subjects and materials, having broad social questions before it. A new department of inquiry, called *Criminology*, has recently been developed, principally in Italy and France, which aims to determine both the social and the individual causes and conditions of crime. The value of such a science must depend essentially upon the soundness of the psychology which it adopts. Hitherto the psychology has been the debatable ground in the entire movement, and criminology has remained the work of individual theorists and observers, while waiting for authoritative collective work upon a sufficiently large number of data. Besides the criminal, other social classes should also be investigated, and the psychologist has an unlimited field for work of great importance in determining the conditions of what has been called "collective" thought and action. The psychology of men in groups, such as the lynching party and the street riot, the reasons for the differences between individual actions of individuals and the actions of masses, the analogies between the performances of what is called the human *mob* and those of minds of lower grades—the animals, human defectives, etc.—these are all important problems. Such a bureau should have at its disposal the resources of a statistical establishment, and command well trained computers to treat the data which are secured. Taken together with Educational Psychology, this department of work makes, in the mind of your Committee, an urgent claim upon the Carnegie Institution next to that of Zoölogical and Anthropological Psychology.

Third. There should be established a well equipped laboratory providing for research in both experimental and physiological psychology—what may be briefly called *Laboratory Psychology*. This laboratory should be so equipped as to provide much of the apparatus required in each of the separate bureaus mentioned. It should be located in a building with the bureaus already mentioned. In this way the expense of equipment would be considerably diminished. In order not to duplicate existing laboratories too much, the equipment should grow gradually as special researches in all the psychological departments may require. For this reason we put this *third* in order.

This department, called above that of Laboratory Psychology, includes two distinct branches of research, both employing exact methods and requiring an equipment of apparatus—Experimental Psychology and Physiological Psychology. By experimental psy-

chology proper is meant the investigation of the mind by experimenting upon normal individuals through the ordinary avenues of sense, or, in other words, through stimulations to the nervous system. It includes the experimental investigation of sensation in all its kinds, namely, vision, hearing, touch, muscular sensation, etc. In this department considerable advance has been made in recent years. Delicate problems, for example, of the theory of vision, including questions of space perception, of color vision, of visual association with other sensations, are now profitably taken up, and standard apparatus for the investigation and demonstration of such phenomena has in some cases been devised. The practical utility of an equipment for experimental psychology, in connection with the Carnegie Institution, would appear in the prosecution of such subjects as that of the investigation of color perception and its defects in connection with colored lights, railway signals, etc., the training of the senses, etc., where at present the greatest confusion and disorder prevails. Citing these as instances, merely, in but one of the departments of sensation, where many of equal importance might be cited, one is able to see the utility of a well equipped laboratory for such research, where, if necessary, the work on any single problem in any department may be pursued.

In physiological psychology, on the other hand, the main problem is to investigate mental conditions in connection with the physiological processes which accompany them, and more particularly in the variations which are presented by accident, disease, etc., or which are directly arranged by the experimenter. Here the science of what may be called neuro-psychology or psycho-physiology has been developed. To cite in this connection also a single line of research, we may refer to the localization of functions in the brain and the remarkable advances in the physiology and pathology of speech. This work has constituted one of the most interesting pages in the history of modern physiology and medicine, no less than a contribution of extreme importance to our understanding of the relations of mind and body. Areas have been located in the brain, as, for, example, that of the function of speech, so definitely and presenting such marked mental symptoms that the surgeon is able by a direct operation to reach the spot, and cure the patient. Our point of emphasis here is that the symptoms are mental, that there are variations or derangements of sensation or movement, as in the case of speech, and the experimental knowledge acquired and the mode of treatment really belong to psychology. The equip-

ment of a laboratory, where direct experiments may be planned to clear up the many problems which are still obscure, would be of untold utility, not only to the sciences of psychology and brain physiology, but also to medical practice.

Fourth. A fourth department of research, that of *Mental Pathology*, is devoted to the investigation of abnormal conditions of consciousness. Its establishment would involve for its adequate equipment so large an appropriation and such extensive resources that it is probably not within our reach at the present time. Your Committee recommends, however, that the bureaus described above for social and physiological psychology should take upon themselves certain more restricted researches in this field. The investigation of the abnormal and defective of certain sorts may very profitably be associated with the gathering of general social statistics. The observation of defective children, for example, in asylums for the deaf, dumb, and blind, and the carrying out of certain tests and measurements upon well determined types of mental diseases in hospitals and asylums would be a work which might well be undertaken, and with profit, by that bureau. Material for such study exists in institutions at Washington, and there can be no doubt that the coöperation of these institutions could be counted upon, as can also that of the schools and, as is said above, of the Government Bureau of Education, for the furtherance of these researches.

These intimations may suffice in the present report as a description of the sort of work which should be undertaken in these several fields. The inspection of this brief list, however, cannot fail to impress the reader with the *need of coördination and of coöperative work by all of these departments under some general direction.* The object of it all is to advance and apply the science of the mind, and that science is most adequately advanced when the results from these different departments are assimilated, digested, and applied in practical life. There should result, therefore, as the work progresses, if it be well done, an appreciable advance of what we call general or systematic psychology. The work of the Institution should show itself in future generations in the theories of the mental life as a whole, in the teachings of instructors, and in the text books used in universities and elsewhere. The important function of the central establishment is, therefore, to be kept in mind in all the work of the departments, and these latter should not be so distinct or locally separate from one another that workers in one of them may not, under suitable conditions, call upon another or be

called upon by the other for aid. In the way of indicating the best results to be reached by the work of all the bureaus in connection with one another, the considerations already presented in favor of a central unifying establishment, with functions such as providing lectures and issuing reports, etc., may be again emphasized.

With so much description of the more general fields of work in which the Carnegie Institution may do most for psychology, we may point out certain subordinate urgent ways in which particular lines of work may be undertaken in connection with the central establishment:

(*a*) A bureau for the *manufacture and sale*, at greatly reduced prices, of all sorts of *apparatus* required by scientific men.

(*b*) A bureau in which there should be provision for the training and support of *expert computers*, who would be at the service of the educational institutions for computing and statistical work. Such a bureau is a necessity in many departments, and a single establishment would suffice for all the sciences.

(*c*) A bureau for *anthropometric and psychological tests and measurements*. The object of this bureau will be the establishing of certain standard tests and measurements, both of psychological and physiological character, to be carried out upon individual classes, such as college students, school children, and primitive peoples, savages, and others in connection with the departments already described. A good beginning has been made by a committee of the American Psychological Association in devising and arranging a set of standard tests.

(*d*) This department should join in the maintenance of a *printing establishment* for the manufacture of scientific works of all sorts at liberal and reduced rates for scientific institutions, societies, and agencies generally, and for its own printing in all the departments.

(*e*) There should be in connection with the central establishment a *literary bureau* for the conduct of such publications as may be necessary. This would include the publication of reports, bulletins and literary aids of all sorts, such as bibliographies, translations, etc. We especially point out the need of an agency for securing the *translation and publication of important foreign books* and memoirs which private publishers do not find it profitable to bring out.

III. Specific Recommendations.

Advisory Committee.—In addition to the officials indicated below for specific duties in connection with the central department of psychol-

ogy, when that is established, your Committee deems it very important that there should be a permanent Advisory Committee associated with the Carnegie Institution for Psychological science in its broadest definition. Such a committee would be a medium of communication between the Board of Trustees and the officers of the Psychological Department. The principal function of this Committee would be to give, in all matters concerning psychology, expert judgment and advice. It would pass upon all proposals of a specific kind which come before the Trustees. It would be competent to take the initiative in making recommendations as to new undertakings which the condition of the science from time to time seemed to render advisable. It would judge as to the qualifications of particular beneficiaries of the funds of the Institution, make recommendations of grants, and suggest appointments in connection with the staff.

It should be charged to find the *exceptional man* and suggest ways in which he may be profitably assisted. In view of these functions, exercised with reference to the entire body of researches in psychology, the chairman of this Committee should be as *broad and tolerant* an expert psychologist as can be secured, rather than one versed only in some one branch of the subject. At the same time the Committee should be numerically small. The chairman may also be responsible Director of the Department.

The chairman should also represent the Committee in conference with representatives of the Board, and also of the advisory committees in the other sciences, so far as such may be constituted. This latter form of conference, namely, among the chairmen of the different committees, seems to us to be of great importance. By such conference the different departments of science would be kept in coöperation, and the benefits of this coöperation would accrue to science generally.

IV. Equipment and Maintenance of Psychological Department, with Estimates of Cost.

In order to be definite in our suggestions as to the most urgent lines of procedure, your Committee recommends the following scheme:

(1) *Building*.—In case there is established in Washington a central institution affording complete accommodations for such of the sciences as find central bureaus necessary, psychology should be

given a special structure, the construction, arrangement, division of rooms, etc., being in charge of one or more expert psychologists.

(2) *Equipment.*—

	First Year.	Second Year.	Annual appropriation.
(*a*) Bureau of Genetic (Zoölogical and Anthropological) Psychology............	$10,000*	$5,000	$5,000
(*b*) Bureau of Educational and Social Psychology, to include Bureau of Tests and Measurements........................	5,000	5,000	5,000
(*c*) Laboratory for Experimental and Physiological Psychology. Over and above provision for building, we recommend an appropriation of..................	10,000	10,000	5,000
Totals.........................	$25,000	$20,000	$15,000

(3) *Salaries and Administration.*—We recommend that certain salaried positions be created for the maintenance of the Psychological Department, distributed with a view to the work of the branches or bureaus mentioned above. The *Chairman* of the Advisory Committee should have a salary, understanding that he is to give attention to all details of grants, projects, etc., which come to the attention of the Institution in psychology. There should be a *Director* at the head of the department as soon as the scheme of bureaus is realized, who should be responsible for its general conduct and administration, who should preferably live in Washington, or, if connected with another institution, should give at least half his time to the affairs of the Carnegie Institution. With him should be associated officers known as *Professors* or *Heads of Departments* or the equivalent, each charged with the control of one of the great divisions of the work as mentioned above, namely, one for *Genetic Psychology*, one for *Educational and Social Psychology*, and one for *Laboratory Psychology*. No recommendation is made as to the institution of a department of Pathology at present. These Carnegie professors, if we may call them so, will be three in number, one of them, however, being already provided for in the person of the Director. In other words, the Director's duties and qualifications should comprise the conducting of one of these three departments.

* This figure assumes the need of constructing some sort of houses, etc., for animals. It may be provided for, in whole or in part, in the Zoölogist's recommendation.

The scheme of expenditure which this arrangement will involve may be indicated as follows:

(*a*) Salary of Chairman of Advisory Committee, $3,000 (besides office and other necessary expenses of the committee, and to be paid only in case he holds no other position in the Institution).

(*b*) Salary of Director, $6,000 or $10,000 (according as he gives a part or the whole of his time). Great flexibility should characterize the arrangements in order to secure the right man.

(*c*) Salaries of two heads of departments or professors at $3,000 or $5,000 (according as they give part or the whole of their time)—$6,000 or $10,000. It would seem quite feasible for a university professor to conduct one of these departments while still serving his university, provided competent assistants or associates be given him.

(*d*) Salaries of staff officers or associates—one or two for each of the three departments, at $2,000 to $3,000 each: First year, $6,000; second year, $7,500; third year, $10,000.

	First year.	Second year.	Third year.
Totals	$20,000 (about)	$25,000 (about)	$30,000

Uniting these two estimates for *equipment and maintenance* and for *salaries*, we have as follows:

	First year.	Second year.	Annual appropriation.
Equipment and maintenance	$25,000	$20,000	$15,000
Salaries	20,000	25,000	30,000
Totals	$45,000	$45,000	$45,000

That is, an expenditure of $45,000 per year, divided as indicated above, will give adequate equipment and permanent maintenance to such a department as we have sketched.

(4) *Special Objects.*—In addition to the foregoing, we recommend the establishment of a fund for certain special objects, the total to be very flexible. The objects to be covered by this fund are, in our opinion, these:

Grants and subsidies	$10,000
Research fellowships (by whatever name they may be known)	6,000
Lectures, publications, etc.	4,000
Total	$20,000

In explanation of this class of special objects, we may say that by Grants and Subsidies we understand particular cases of research

which the Advisory Committee may find it wise to recommend. This would include all applications which come to the Institution and are favorably considered under the terms of the deed of trust.

By *Research Fellowships* your Committee intends the establishment of certain bursaries, to be awarded to mature and most capable individuals, to enable them to carry out important researches at the Institution or elsewhere, as may be determined, for one year or a term of years. These should be awarded on recommendation of the Advisory Committee. It is thought that six such bursaries, yielding $1,000 each, will, at any rate for a certain number of years, be sufficient.

The third item, *Lectures, publications, etc.*, is one which your Committee recommends with a view to adopting means of furthering in a public manner the interests of the science. We think it would be extremely helpful if experts should be secured to give at intervals, in Washington, lectures devoted especially to topics of the interconnection of the sciences and of different branches of science, with *résumés* of the progress of science, and to topics which would, in general, serve to unify the work of the Carnegie Institution. This should be undertaken in coöperation with other departments.

Summing up the recommendations made in this section of the report, we reach the following condensed statement:

Recommendation for equipment, maintenance, and salaries, as given above, annually	$45,000
Recommendations for special objects, as given above	20,000
Total annual appropriation	$65,000

The total recommendations thus involved amount to from $60,000 to $65,000. The maximum sum recommended, namely, $65,000, is, in the mind of your Committee, by no means excessive.

(5) *Partial Schemes.*—Partial, or lesser provisions, in case reduction is imperative, could be carried out by omitting one of the principal bureaus or departments. In this case the *Equipment of the Laboratory* may be reduced to the essentials of the special bureaus and many researches undertaken without such full equipment. This gives a reduction in salaries and equipment to the amount of $10,000 a year. This reduction would make the entire requisition for psychology $50,000 per year (when the whole scheme has been put in operation).

V. Immediate Procedure.

With a view to proceeding as financial and other conditions may allow, on the lines now suggested, your Committee makes the following recommendations in regard to immediate procedure:

(1) It is urgent that a permanent Advisory Committee be constituted, as already indicated.

(2) An appropriation of $5,000 is recommended for the preliminary work of a bureau of *Psychophysical Tests and Measurements*; to extend and apply the work already done (by a committee of the *American Psychological Association* and by private individuals) principally upon university students. The procedure should be left to the Advisory Committee on Psychology.

(3) An appropriation—jointly with Zoölogy—for the study of animals. Immediate procedure is strongly recommended, a joint recommendation being secured from the Committees on Zoölogy and Psychology.

(4) Special grants as per report on specific applications.

We present herewith, in support of the general lines of our recommendation, copies of letters giving the views of certain psychologists—themselves representative men, who also represent institutions having important psychological departments.

These letters were sent to your Committee in response to the chairman's request, with which in each case a list of suggested undertakings was inclosed. A copy of this list is also appended. These letters give the judgment of leading authorities as to the relative availability of the undertakings upon which the Carnegie Institution might enter in this department. It will be seen that the opinions expressed are in the main coincident with the recommendations made in this report, and also that such a scheme as that now suggested is, in their opinion, not at all in conflict with the interests of their university departments. More than that, in their opinion, the institution of a central establishment for psychological work in Washington will help and stimulate, rather than compete with and discourage, the psychological work now being done in the universities.

Respectfully submitted.

J. Mark Baldwin, *Chairman.*

October 31, 1902.

Appendix to Report of Committee on Psychology

[*Projects in Psychology on which certain expert opinions were asked.*]

1. Unification of departments of psychology; bureau for lectures, conferences, etc., in Washington.
2. Laboratory for experimental and physiological psychology in Washington.
3. Bureau for educational psychology, adjunct to above.
4. Bureau for tests and measurements, adjunct to above.
5. Station for the study of comparative psychological undertakings in connection with biological experimental farm.
6. Bureau for statistics and compilation to train and supply expert computers to all the institutions.
7. Bureau for the study of mental pathology, adjunct to an establishment in general pathology.
8. Establishment of fellowships for research, such as fellows to work in the several bureaus in Washington.
9. Grants and subsidies to particular individuals for research.
10. Preparation of a bibliography for psychology.
11. Establishment jointly, for all the sciences, of a bureau for cheap printing, lithography, etc.
12. Possible provisions for philosophy and logic (no definite proposals have as yet been made).

From the replies to requests for opinions on the above projects the following have been selected to form a part of the report of the Committee on Psychology.

[*Dr. George Trumbull Ladd, Professor of Philosophy, Yale University, to Mr. Baldwin.*]

The purpose and opportunities of the Carnegie Institution are so unique that there is little in the previous history of education and scientific research which can serve as a very exact model or altogether safe guide. I think, therefore, that its first years should be very largely experimental; that the Institution should, so to say, feel its way cautiously to its best and most effective service for the advancement of knowledge and the practical benefits of increased knowledge, in this country and throughout the world. Its efforts, so far as concerns the aid it can render to psychology and to the psychological sciences should, in my judgment, be arranged in the following order of precedence:

1. The unification and supplementing of the work of research done by the universities and individuals best equipped for such work. This branch, however, can not be defined in particular at the beginning. But as instances of definite assistance in the direction of unification I will mention the following:

(*a*) The creation of some sort of a bureau of information, so that the different persons engaged in psychological researches can know what is being done, and what has been done, by others toward the solution of the same problems. It has been my experience that much time and energy are often wasted through failure to obtain such information.

(*b*) Some arrangement by which friendly criticism and suggestion can be had with a *minimum* risk from those jealousies, misunderstandings, and even misrepresentations from which it is so difficult to keep free even our best scientific work.

(*c*) Perhaps, still further, more adequate provision for the efficient distribution of apparatus, books, pamphlets, etc., that are either necessary or helpful to the investigator, but which he could not obtain without the assistance of the Institution.

Such ways of unifying as these are, of course, at the same time ways of supplementing the work of particular universities and of individuals.

2. Some sort of a central plant seems to me to be the next most pressing need. I should not, however, think it wise to spend immediately a large sum in buildings or in the purchase of expensive apparatus for experimental purposes. Indeed, a certain rather limited amount of space allotted to the uses of a psychological labo-

ratory might be, and I think would be, all that is desirable for some years to come. The most important equipment for such a laboratory is—

(*a*) A workshop in which apparatus of the most useful sort, and perhaps also some of the highest quality of finish and exactitude, can be manufactured.

(*b*) This workshop will be comparatively useless unless there are two or three mechanics connected with it who can have the very special training which such manufacture involves.

(*c*) Some oversight from a trained expert in this branch of psychological research.

Besides the workshop, the laboratory should have a few rooms which might be from time to time, at no great expense, adapted to the uses of those conducting researches at the central building of the Institution.

The uses to which the laboratory should be put are:

(*a*) The providing of those who are conducting researches of a large general sort—*e. g.*, in anthropological or pedagogical lines—with the necessary equipment of apparatus for their experiments. Such experiments imply that the traveling expert takes his mechanism along with him.

(*b*) The conductors of pieces of research in the rooms of the laboratory, or in the city of Washington, can then have their apparatus constructed, tested, and readjusted under their direction.

(*c*) By and by, if not soon, the laboratory of the Carnegie Institution might be able to supply experimenters in this country with as good mechanism as can now be obtained in France or Germany, of the special sort adapted to experimental psychology, at cheaper rates, or even by way of a loan.

Beyond these uses I do not deem it wise for the laboratory of the Carnegie Institution to aim to go until at least the rather delicate problem is practically solved of its really supplementing rather than harming the work of the laboratories of the universities.

3. Next in importance I should place the commissioning and equipping of experts in the matter of making psychological observations, tests, and measurements, computing results, etc. No serious and safe work in anthropology, or even in certain lines of antiquarian research, can possibly be accomplished in these days without the assistance of one or more experts in psychology. No anthropological commission should be sent out by either the Institution or the Government of the United States unaccompanied by such an

expert. The delicacy of making any device and the scandal of making a mistaken choice, and the almost greater scandal of trying to advance anthropological researches in neglect of the work of the psychologist, are additional reasons for the Institution to lend a strong, helpful hand here. The same thing is true if we are ever to have any worthy results from the experimental study of our most fundamental and pressing problems of the education of our public schools. Again, in many cases of more private researches the handling of the data obtained by some expert computer or their review by some one skilled in psychological tests and measurements is necessary to determine their value.

4. In the fourth grade of importance, if not even higher up in the scale, should be placed the aiding of individuals in researches where the expenditure of time, talent, and money makes it difficult or impossible for such individuals to conduct, without aid from the Institution, their researches successfully. These grants to individuals should, of course, be made very judiciously, and even sparingly, as respects numbers. They can probably never bear any large proportion to the number of requests for grants; but in certain cases they will need to be generous in order to be effective; and it must be borne in mind that the true investigator does not always know, by any means, just where he is coming out or what point his investigation may produce.

5. I do not favor inaugurating at present any system of fellowships. In my judgment, the entire business of fellowships has been quite overdone by our universities. In certain rather rare cases very promising young men might perhaps be sustained in their researches, either wholly or in part, by the Carnegie Institution. In general, however, only a percentage of those now enjoying such assistance are really worthy of encouragement for the higher purposes of even university work. From the ranks of some of our younger *teachers* I should think occasional Fellows of the Carnegie Institution might be temporarily drawn off.

6. Later on, and after these more immediate needs are met, the Institution might profitably use its large resources in fostering the study of animal and comparative psychology.

7. At the same time, or perhaps earlier, if funds are available, and, what is harder to secure, men who are competent, work in pathological psychology and in the investigation of defective children, etc., might also be undertaken.

In both these lines of work I think that the movements of the

Institution should be very slow and cautious, except in so far as they can assist in such work as it falls under the heads of the proper functions of the Institution, as enumerated above.

8. Printing and publishing the results of research and the reports of commissions and committees should, I think, be done by the press of the Carnegie Institution, if it is deemed wise to establish such a press. I should favor its establishment.

9. Occasionally, perhaps, but only very rarely, should the Institution assist individuals in publishing books which are not prepared as part of the Institution's work.

10. I do not favor the plan of having the Carnegie Institution compile or assist in compiling a bibliography of psychology.

In closing I wish, as the head of the department of psychology in Yale University and in behalf of my colleagues, to express our grateful appreciation of the magnificent gift of the founder of the Carnegie Institution, our hearty sympathy with his general purposes, and our willingness to coöperate in carrying to success these purposes, so far as fidelity to the university renders such coöperation possible.

[*Dr. Hugo Münsterberg*, *Professor of Psychology*, *Harvard University*, *to Mr. Baldwin.*]

PROPOSITIONS FOR THE USE OF THE CARNEGIE FUNDS IN THE INTEREST OF PSYCHOLOGY.

(Nos. 1–7 to be Established in Washington.)

Most Valuable.—1. A subsidized printing establishment for monographs in psychology and other sciences, which would print and prepare plates on a commercial basis, for any customer, at a rate comparable to that ruling in Germany or France.

2. A subsidized mechanical establishment for the construction of psychological and other instruments at European rates of expense.

Very Valuable.—3. An institution for psychological experiments on men, with special emphasis on such problems as can not be easily studied in the usual university laboratories. Here belong—

(*a*) Experiments on the influence of abnormal conditions which as such are undesirable in an educational institution.

(*b*) Experiments which demand more time than the university students can afford to give.

(*c*) Experiments which need a more elaborate equipment than the universities can afford.

(*d*) Experiments which demand a large number of subjects. Necessarily such an institution would not only appoint conductors to assign problems and methods, but also award fellowships to highly advanced young scholars, who could serve as self-observing subjects of the investigations. In this respect such an institution would be different from a physical or chemical institute, and distinct also from the two following.

4. An institution for animal psychology. Here belong—

(*a*) Experiments on such animals as by their size or habits of life cannot be kept in the regular university laboratories.

(*b*) Studies in heredity.

(*c*) Experiments on animals under abnormal conditions.

Valuable.—5. An institution for anthropological-psychological measurements and statistics.

6. A bureau for the exchange of psychological observations in the laboratories of the country (provided that no attempt is made to control the various laboratories), for computing results which any psychologist may send, for compiling literature, for giving information on literature to psychological investigators, etc.

7. An institution for pathological psychology, working in connection with an asylum.

Valuable but Very Dangerous for the Development of the Educational Life of the Whole Country.—8. Aid to individual scholars who are acknowledged to be specialists of merit, in the form of aid for instruments, assistance, printing expenses, leave of absence—all this on application of the scholar.

9. Aid for young, promising doctors of philosophy, in the form of fellowships for work to be done independently of the institutions to be created in Washington; such aid to be given merely on application from the academic teachers of the recipients.

[*Dr. James McKeen Cattell, Professor of Psychology, Columbia University, to Mr. Baldwin.*]

If the Carnegie Institution establishes laboratories in Washington, I should place first a psychological laboratory for the investigation of problems requiring exact methods. This laboratory might supervise—

(*a*) Printing press and instrument shop in connection with the other sciences.

(*b*) Bureau of computations in connection with the other sciences.

(*c*) An anthropometric institute in connection with anthropology.

(*d*) An institution for tests, etc., in schools, in conjunction with the Bureau of Education.

(*e*) An institution for work on the defective classes, in conjunction with asylums, etc.

(*f*) An institution for psycho-physiology in conjunction with work in physiology.

(*g*) An institution for comparative work in conjunction with biology and a zoölogical park.

(*h*) A pathological institution in conjunction with work in psychiatry.

(*i*) Bibliographic work and a library.

This is in general the order of my preference, though I think that the direction of work should depend chiefly on the men and facilities at hand. I can not well place in order of merit the ways in which the Institution can help psychology throughout the country. I have discussed the matter in detail in *Science* and can not give a concise list without danger of misunderstanding.

[*Dr. Joseph Jastrow, Professor of Psychology, University of Wisconsin, to Mr. Baldwin.*]

1. I am of the opinion that the most decided aid would come from the establishment of a research laboratory in psychology, or, next to that, of research endowments for existing laboratories in general accordance with my suggestions in *Science*. While there are many psychological projects which it is of great importance to further, it is, in my opinion, still more important to attract the energies of psychologists into the research field by establishing positions in which capacity for research is the main requirement, and by transforming the status of their present positions so as to make possible an emphasis of the research side of their profession. My first answer would then be to aid research in psychology by supplying personal assistance to men who have the promise of doing something noteworthy, giving grants for clerical aid, for computers, for statistical expansion of investigation. Aid men so that the time they do spend on research shall be most efficiently spent. The endowment of specific projects, though important, is to me not so important as this more difficult, but in the end more efficient, stimulus.

2. I am willing to express my opinion that there is room in Washington for a psychological position of wide influence. The Bureau

of Education distinctly needs the services of a psychologist. In certain aspects of the work of the Bureau of Ethnology, of the Surgeon General's Library, of the Smithsonian Institution, and of the National Museum, the coöperation of a trained psychologist would be distinctly helpful. It should likewise be borne in mind that there are in Washington and its vicinity educational institutions furnishing opportunity for the engagement of a psychologist of high standing in advanced instructional work.

3. Of specific problems worthy of encouragement I bring forward a few which have engaged my special attention:

(*a*) A commission for the invention, examination, and establishment of mental tests and the dissemination of the laws, so that the normal endowment of man in regard to his fundamental psychological equipment may be determined. An important factor of the problem consists in the investigation of the correlation of such functional efficiency with developmental changes. Such *normals* would find further application in the study of abnormal variations; in the determination of the correlation of the several avenues and types of mental endowment with one another and with physical capacities; in the practical application to educational methods, and in other directions less readily specified.

(*b*) A special study of the psychological status of the processes most fundamental to elementary education. Speaking, reading, and writing form one group of these; the associative processes of mathematical calculation and logical inference form another. The object is to furnish a positive basis upon which education may rely for deciding between rival methods and to terminate the endless and haphazard discussions that form so large a part of educational diversities of opinion.

(*c*) The provision for the psychological study of abnormal mental traits, both in connection with insane asylums, the institutions for the defective classes, the examination of special individuals, etc. The purpose of this would be to give a more adequate formulation to the psychological side of the care and treatment of the mentally defective and to furnish the means by which the many false and misleading notions and observations now current in regard to such extreme mental variations might be more successfully combated. Psychic epidemics and popular movements of a promiscuous character can be opposed most effectively by providing an authoritative statement, to which those who instruct the public could readily turn.

(*d*) A station for the strictly psychological study of animal intelligence is of critical importance for the further advance of comparative psychology.

I have confined my enumeration to a few of the projects of larger scope which a central establishment for research could most suitably direct, and which are less likely than others to be taken in hand by existing facilities for research.

4. Of coöperative measures I should regard as most helpful—

(*a*) A central scientific instrument works, in which research apparatus could be brought to perfection, and from which standard apparatus could be issued.

(*b*) The endowment of bibliographical aids to the student of psychology, and the provision of adequate representation of psychology in more general bibliographies.

(*c*) The provision for the representation of psychology in anthropological expeditions, so that the mental status of the various races may be established. Such opportunity is in many cases likely to disappear, owing to the commingling and extinction of peoples.

(*d*) The establishment of branch depositories for the gathering of tests of normal mental efficiency in connection with the project outlined in 3 (*a*).

[*Dr. Edward Bradford Titchener, Professor of Psychology, Cornell University, to Mr. Baldwin.*]

1. On the assumption that psychology is to benefit by the Carnegie fund, the question at once arises whether the sums allotted to psychology should be spent in a lump, for one purpose, or whether they should be distributed for various ends. As regards psychology itself, the question arises whether we are concerned to further the immediate needs of the science, to hasten progress along lines already laid down, or whether we desire to erect a permanent monument, which may lie a little outside of our visible needs, but which will be of enduring value, whether psychology advances along its present track or undergoes such another revolution as it suffered with the introduction of the experimental method.

On the former issue, my own opinion is that we should either effect a compromise or boldly put the whole fund into commission. I am not in favor of a single lump-sum expenditure. On the latter I think that the immediate needs of psychology are pressing; that we now require, not a few geniuses, but rather a large body of

capable, rank-and-file workers, who shall thoroughly work out the multitude of problems handed down to us from the second half of the nineteenth century; that the work before us, on lines already laid down, will occupy at least two human generations, and that it would be unwise to allow for or to try to envisage the progress of psychology beyond this point.

These matters are, however, extremely debatable. Fortunately, the present conditions of psychology practically dictate to us, irrespective of our personal beliefs, what is the first thing to be done with a Carnegie grant.

2. We must, in the first place, have a share in a subsidized establishment for printing, engraving, lithography, etc. There should be no interference with the integrity of existing journals; but we ought to be able to have articles prepared for publication, on the mechanical side, as cheaply as is possible on the continent of Europe. The establishment should have skilled proofreaders, such as are attached to the larger German printing houses—technically trained readers. I have not myself felt the need of trained computers and statisticians, such as are required in some forms of genetic work and in work on mental tests, but I see no reason why this same establishment should not have a staff of computers also. The need of them is certainly pressing in some departments of psychology. At any rate, the need of printers and engravers is imperatively pressing in all departments.

I am, then, entirely clear on the point that, whether we are to have one thing or many, we must have this. It is now hardly conceivable that our share in the establishment will exhaust our share of the funds. What is to be done with the rest?

3. If the funds admit, I should here make the compromise spoken of just now. I should sink all the rest of the money in a central psychological institution; but it must be understood that this institution is not to duplicate, or slightly to improve upon, existing laboratories. It must be an *over-laboratory*. It must be able to perform work which for any sort of reason—time, expense, difficulty, number of observers, necessity of pathological material—can not be performed in a regular university laboratory. It must make full and adequate provision for work in comparative psychology, perhaps by granting the use of zoölogical collections, perhaps by way of an experiment farm, perhaps by arrangements with existing biological laboratories, or by all of these means together. It must be representative of the whole of psychology; which implies that, while it

is to have a permanent administrative staff, its direction must change, on the scientific side, every three or five or so many years; no single man is nowadays representative of the science. It must have inducements to brilliant young men, in the way of fellowships given for three or five or some good round term of years—competitive fellowships, strictly limited in number. It must have a full collection of historical instruments; a museum, with descriptive labels and references, always open to investigators from elsewhere; perhaps a loan collection of elaborate pieces; perhaps a workshop, where instruments could be procured by the university laboratories at cost. To be complete, it should have a library and a bibliographical establishment; though I regard these two items, under existing conditions, as of minor importance.

Such an institution would be both of immediate and of permanent value to psychology. I greatly doubt, however, whether the idea can be realized. There are many sciences, and the Carnegie fund is limited. Much saving might be effected by affiliation with existing zoölogical gardens, insane asylums, biological laboratories, etc.; but I should regard the narrowing of the material equipment of the Institution—anything that tended to make it a name or a bureau—and the scattering of the men connected with it as exceedingly dangerous. To do the work which I have in mind, the Institution should be as imposing materially, by its block of buildings and its centralized staff, as morally by its purpose and program. It should be, literally, a central station for psychology. While availing itself of local opportunities all over the country, it should bring together, for a part of each year, the best men in all departments of psychological inquiry. It should be a visible witness to the range and diversity of psychological problems and interests. Anything in the nature of a halfway house or a first beginning I should look on with grave suspicion.

If, as I suspect, there is no prospect of realizing such an institution as I have here sketched, I should recommend the use of the Carnegie grant for the purposes named in my *Science* paper, namely:

4. Valuable fellowships, of $750 or $1,000 for two or three years, granted to doctors of philosophy of acknowledged power and merit; these fellowships to be held at existing institutions, at the choice of the appointees; living wages, of $300 or $400 for one year, granted to promising graduate students who are too poor to pay their own way; grants of $500 to $1,000, made to professional psychologists, without demand of program or promise of result, on their personal

guarantee (backed, perhaps, by a committee of their colleagues) to expend the money for the advancement of science in some fairly definite direction. I propose these things as an alternative to the creation of the central institution. They do away with any need of or excuse for an expensive central establishment in Washington. I should prefer the institution. At the same time, I do not see any general danger to the cause of education in the creation of these three forms of subsidy, granted practically to existing universities.

The list of *Projects in Psychology* contains twelve sections.

No. 11, a printing establishment, I put first.

Nos. 1, 2, 4, 5, 6, 7, 8, 10 I have dealt with, in order of their apparent merit, under my discussion of the central institution.

No. 9 is covered by my alternative proposition to that of a central institution.

Nos. 3 and 12 appear to me, however valuable in themselves, to lie outside of psychology proper.

REPORT OF ADVISORY COMMITTEE ON HISTORY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The undersigned, requested by you to serve as an advisory committee in matters of history, beg leave to submit the following report, based upon a careful consideration of the present status of historical studies in the United States:

Introductory.—While we think it probable that ultimately considerable sums may well be expended, under adequate methods of supervision, in directly aiding the work of individual investigators along special lines marked out by themselves, we make no recommendations of that kind at present. We believe that results permanently and widely valuable to historical science are much more likely to be obtained by devoting attention first of all to the promotion of general and comprehensive yet definite projects, helpful to the profession at large. We should give the foremost place to certain tasks naturally fundamental, tasks which logically come first as the necessary preliminaries to generations of successful individual work. It seems to us clear that, for some years at least, attention should be mainly concentrated upon American history, European history being relatively well cared for already by existing European agencies. We also believe that it is well not to make initial recommendations or arrangements too elaborate, experience showing that it is wiser to give scientific establishments an opportunity to grow in the hands of those who direct them.

So far as we can judge from such consultations as we have been able to have with historical students, the principles above stated seem to have the general approval of the profession.

On these general principles we make three recommendations:

1. *An Institute of Historical Research in Washington.*—More American historical work of wide utility can be done in Washington than anywhere else. It is desirable to found there a permanent establishment under a competent director, who should be given some freedom to develop it. There are goods models, *mutatis mutandis*, in the French, Prussian, and Austrian institutes at Rome. Such an establishment could serve four important purposes:

(*a*) *First* of all, in logical order, it should execute a comprehensive and detailed examination of the Government archives, resulting in

the preparation of a monumental report upon the vast store of manuscript materials for American history now preserved in Washington.

(*b*) *Secondly*, it might thereafter proceed, upon a carefully considered plan, to the scholarly editing of whatever might seem most important in this mass of historical material. If a bureau maintained by the Carnegie Institution (in which Congress will, no doubt, have the same kind of confidence as in the Smithsonian) should undertake to provide such bodies of documentary material, prepared in the best manner for publication, there can be little doubt that Congress could readily be persuaded to provide for their printing. The result might be a noble series, comparable to the *Monumenta Germaniæ Historica* and other collections of sources achieved in Europe by the mutual coöperation of governments and scholars.

(*c*) *Thirdly*, such an institute should serve as a clearing house for the historical scholars of the country. It should facilitate their personal researches in Washington and, so far as possible, aid those who are at a distance to avail themselves of its treasures.

(*d*) *Fourthly*, it should provide suitable guidance and instruction for such advanced and highly competent graduate students as should resort to it for the purpose. The number should be kept small by high requirements. But it would be of great advantage to the future of historical instruction and investigation in this country if every year a few of the best and most advanced graduate students of history could, before they begin their life work, be properly introduced to the rich stores of historical material in Washington and to its inspiring atmosphere, and, it should be remarked, there exists now no regular means toward this end, such as the scientific establishments of the Government afford to students of the physical sciences. In such instruction the director should have the aid, perhaps by annual rotation, of a succession of professors from the leading universities.

To proceed cautiously and to make a right beginning are so important that it might probably be best that the director chosen for such an institute should, for the first year, or at least for some months, occupy himself solely with such inquiries, examinations of archives, and consultations in Washington and elsewhere as should enable him to devise judicious detailed plans for its organization and early operations.

We judge that to obtain the service of the best man for such a position the Carnegie Institution should be prepared to pay a salary of $5,000 per annum; that $3,000 more should be appropriated to pay each year the professor who should teach on leave of absence from his university; that the assistance of younger men and of a typewriter would be needed in the work of the institute to an extent represented by about $2,000 a year, and that there would be need of an office, a small seminary room, and a small library of historical books of reference. An annual expense of $12,000 might be reckoned upon as adequate; less would suffice in the first year.

2. *A Search of European Archives.*—Vast masses of material for American history exist in European archives. Incomplete examinations of them, partial reports upon them, have abounded, and scholars have used them, but for the most part casually and without the possibility of being certain that their searches were exhaustive. Good logic, good sense, and the example of the best European practice would alike dictate that inventories should come first, and exploitation afterward; that we should first find out what material there is, and then lay plans for using it. We therefore recommend that steps be taken toward a thorough examination of the archives of Europe, with a view to comprehensive and detailed inventories of the materials which they possess for the history of the United States and its various parts and dependencies. It should include a search of national, and in some cases of provincial, municipal and family archives; of the archives of the Vatican; of the great religious orders; and of other ecclesiastical bodies and officials formerly holding sway in any part of America. Especial attention should be paid to the repositories of historical manuscripts in Spain relating not only to the history of the United States, but to that of our new possessions, which can not be properly managed without a completer knowledge of their previous development.

There are two possible ways by which such an inquest might be conducted. It is possible that a competent supervisor might be engaged to reside in Europe continuously until the inquiry is finished, to employ proper persons to make searches simultaneously in each country, and to push the task to a speedy conclusion. But practical difficulties of detail might work against this plan. The Americans best fitted to search in various countries respectively, though usually able to secure an occasional year in Europe, might not be able to be abroad simultaneously. In that case another plan would be preferable, though slower in covering all Europe. According to this,

without employing a general organizer for all Europe, the problem could be taken up country by country. The man most competent to search for and inventory American materials in Italy, for example, having been selected, he could be sent there the first year in which he was free to go. The searcher should, of course, be provided with proper assistants, manual and scholarly. It is possible that, as in Washington, some of the features of a training school could be combined with the work of exploration. The searchers might take with them properly qualified graduate students and use them as apprentices.

But the countries of Europe differ so widely one from another in the profusion, character, and accessibility of their materials for American history, and in the extent to which these have been catalogued and used, that we do not believe it possible at this distance and in the present state of our knowledge to decide off hand upon that plan of operation in the examining of archives which shall in each country lead to the completest information and the most useful form of inventory. We therefore recommend as the first year's work in this field that as soon as it is practicable the Trustees of the Carnegie Institution engage for one year the services of an accomplished American historical scholar, already well acquainted with several European archives, who shall visit the various states of Europe, collect information concerning conditions, persons, and methods, and report a comprehensive plan, adapted to the circumstances of different countries, for the conduct of the detailed inquest above described. He should have proper clerical assistance. We suggest an appropriation of $5,500 for this preliminary survey—$4,000 for salary and traveling expenses and $1,500 for clerical assistance. To what extent and in what directions the work in Europe should ultimately grow and what its future needs would be we do not now attempt to prescribe.

3. *The American Historical Review*.—The *American Historical Review* is at the present time the chief means for the publication of brief historical researches. As such it is doing a great service to American historical scholarship. It is, in a quite exceptional sense, the constituted organ of the profession. It was founded by a union of all historical interests. It is owned and managed by a board whose members are professors in various universities, and who are elected by the Council of the American Historical Association. That Association, without seeking to influence the policy of the *Review*, gives it an annual grant as large as it can afford. With

this aid it has barely succeeded in paying its expenses. But it has done so only by using a guarantee fund raised at its inception and now nearly exhausted, and by reason of the fact that, while it pays three dollars a page (and the book) for reviews of books, it pays nothing for "body articles." Its quality as an organ of American historical scholarship would in our judgment be greatly raised, with valuable results to scholarship itself, if it could also pay four dollars a page for articles. This would enable it to command articles from the best specialists or the leading professors of history. These are often, instead, writing for journals that pay, but would rather write the kind of articles which the *Review* desires, the fruit of solid researches scientifically presented, if they could get something for them, though it were less than the popular magazines pay. To enable this to be done, and to avoid the deficits hitherto met by the use of the guarantee fund, we recommend a subvention of $2,000 per annum to the *American Historical Review*.

If such a relation were established with the *Review*, the historical department of the Carnegie Institution could undoubtedly use a section of the journal as a means of direct and regular communication with the historical students of the country. Whatever modes of publication the Carnegie Institution might adopt for its general purposes, it would be a distinct advantage to have quarterly bulletins concerning the work of its Institute of Historical Research in Washington, or concerning the progress of its work in Europe, inserted in a journal which goes to all members of the American Historical Association and is read by all historical scholars in the United States. Constant interest of the special public in its historical activities would thus be insured.

There are several other objects of historical expenditure which appeal strongly to us, such as the work of the Historical Manuscripts Commission and the Public Archives Commission established by the American Historical Association, and the necessary researches for a scholarly atlas of American Historical Geography or a Dictionary of National Biography comparable with the English. But these either are going slowly forward, with money supplied by the Association, or can wait. We conclude to emphasize, as of primary importance to American historical scholarship at the present time, only the three objects we have described above.

The appropriations which we recommend are: For the Institute of Historical Research in Washington, $5,000 the first year, $12,000 thereafter (or from the completion of the director's plans for organ-

ization, which might be completed in less than a year); toward the preliminary search of European archives, $5,500 in the first year; for the *American Historical Review*, $2,000 per annum.

Respectfully submitted by

J. FRANKLIN JAMESON, *Chairman*,
CHARLES FRANCIS ADAMS,
A. C. MCLAUGHLIN,
Committee.

NEW YORK, *October* 25, 1902.

REPORT OF ADVISORY COMMITTEE ON MATHEMATICS

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Mathematics begs leave to submit the following report:

General Statement.—Mathematics, though abstract and bearing the closest relations to pure logic, is most intimately connected with all the physical sciences, and through the rise of the statistical method is becoming of increasing utility in the other sciences. In this country mathematical research is thoroughly alive and full of promise, especially along various lines of pure mathematics and celestial mechanics. As to the future, it is certainly desirable that more men, with full appreciation of modern mathematical principles and processes, should devote themselves to investigation in the various natural sciences.

In the applications of mathematics, statistical methods stand at present in the foreground. They would seem to have a future in the problems of meteorology, biology, and other branches of science. Their natural use arises in connection with attempts to put theories such as Darwin's to a direct test, and in investigating possible interrelations between one set of physical data and another set. Such inquiries, to have value, require the coöperation of mathematicians who know exactly the true relation of the mathematical machinery and of experts in the particular science; for the mathematician by himself lacks the quasi-instinctive recognition of absurdity in erroneous results, and can not estimate the value of the data; and, on the other hand, the expert by himself is apt to regard conclusions as generally true which are based to some extent on assumptions introduced to simplify the mathematics.

Thus, for instance, in meteorology and terrestrial magnetism there are enormous accumulations of data, and the investigation as to the existence of certain cycles and the sequence of weather types is of obvious importance. Thus there is a wide field for statistical investigation of a kind that calls for skilled mathematical power, as well as special insight in the subject matter; and the same is true, no doubt, in biology and anthropology and economics.

These considerations point to a Bureau of Statistics.

The American Mathematical Society.—The mathematical interests of this country are already fortunate in that they have secured or-

ganic expression in the American Mathematical Society. This society, organized in 1889 as the New York Mathematical Society, rapidly became in fact a national society, and was recognized in 1893 as the American Mathematical Society. It embraces in its membership, almost without exception, every working mathematician in this country and also a number of European mathematicians. The policy of its management has been one of consideration for all phases of mathematical activity. Its *Bulletin* was published from the beginning as a monthly journal of historical and critical character. In 1900 publication of the *Transactions* of the society was begun, a group of ten universities temporarily supporting the society in the financial burden. With the beginning of the publication of the Transactions the society has undertaken to complete its library and transform it into a valuable circulating library, available for the use of its members and all other mathematicians.

The activities of the society are set forth in more detail in the subjoined letter from its secretary. In our judgment, any additional income which may come to the American Mathematical Society by way of subvention from the Carnegie Institution will be well used by the society to promote mathematical research in America. We recommend in particular that for a term of years $1,000 annually be granted to the society for the purpose of converting its library into a thoroughly good collection of books and models, to be placed unreservedly at the service of the working mathematicians of the country.

American Journals of Research in Mathematics.—Many leading mathematical journals of Europe receive subventions from government sources. We recommend that there be granted subventions to the following journals of this country:

	Per annum
American Journal of Mathematics	$1,000
Transactions of the American Mathematical Society	1,000
Bulletin of the American Mathematical Society	500
Annals of Mathematics	500

These four journals are most powerfully promoting the interests of mathematics in this country, and they would be enabled to become still more valuable factors by the receipt of such subventions.

The very considerable labor of editing these journals, as things stand, falls upon men who as leaders in research have become prominent in the great universities—that is, upon men whose energies are already heavily taxed.

The ideal editor of a scientific journal should exert, especially upon the contributors just entering upon their career of research, an extremely valuable educational influence. Hence, if possible, he should be set free for that work. To accomplish this he should receive all the assistance possible that can be performed by a younger mathematician in a clerical way, such as expert proof reading, etc. Furthermore, as a result of the rapid development of mathematics in so many different directions, it is impossible for any one editor satisfactorily to judge of the merits of the manuscripts sent to him, and he is compelled to seek aid from specialists of established reputation as referees. The work of these experts requires time and energy, which it is unjust to demand without payment therefor. The present limited incomes of the journals preclude such payment, and thus there result much delay and other loss of efficiency.

Treatises of Advanced Character and Collected Works.—The European academies arrange to publish the collected works of distinguished scientists. In England the great university presses perform the same function, and, furthermore, facilitate the publication, even at a financial loss, of treatises of high scientific character. It seems fitting that the Carnegie Institution should perform a like function in this country.

We earnestly recommend that the Carnegie Institution arrange for the collection and publication of the works of George W. Hill. We estimate the expense at $2,000 per year for six years.

We furthermore recommend the publication of the collected works of Euler, which would constitute a thesaurus of the mathematics of the eighteenth century. The expense is estimated at $3,000 per year for twelve years.

Grants to Individuals in Aid of Research in Mathematics.—It should be understood that applications may be made to the Carnegie Institution for aid in specific researches; for example, in the preparation of tables or of bibliographies or of reports, and in the construction of new models or mechanisms; but that applications for aid in study without defined productive purpose are not desired.

A most obvious way of promoting research would be to give more freedom or facilities for research to those men or groups of men who through recognized original power are professors in the great universities. One wise form of aid would seem to lie in attaching especially able students who have taken the doctor's degree to such leaders in original research as *research assistants*, an arrangement serving at

the same time to increase the activity of the older men and to establish in the younger men a habit of research.

We set down in conclusion a *summary of the specific recommendations of this report:*

Subventions in aid of publication: Collected works—G. W. Hill, for six years, $2,000 a year; Euler, for twelve years, $3,000 a year.

Mathematical journals of reseach as listed above, $3,000 a year.

Subventions in aid of research: Toward the establishment of a lending library of books and models, $1,000 a year.

Respectfully submitted.

E. H. MOORE, *Chairman*,
FRANK MORLEY,
ORMOND STONE,
Committee.

NOVEMBER 5, 1902.

[*Letter from F. N. Cole, Secretary American Mathematical Society, to Prof. Morley.*]

501 WEST 116TH STREET, NEW YORK,
August 28, 1902.

MY DEAR PROFESSOR MORLEY:

In reply to your recent letter in which you suggest an expression of my views in regard to the distribution of that portion of the Carnegie fund which may be assigned to mathematics, I should like to present the following considerations:

Assuming that other mathematical interests of the country will receive deserved recognition, I shall confine myself to a presentation of the claims of the American Mathematical Society, and the discussion of the manner in which any funds which it may receive may be most efficiently employed.

If the Carnegie fund is intended to help those who can help themselves, then the record of this society entitles it to most liberal recognition. For fourteen years it has been the center of mathematical activity in this country. Including in its membership nearly every American mathematician of any standing, it represents their organized strength and interests. It has stimulated their great advance and been stimulated in return until its overwhelming prosperity taxes its administrative resources to the utmost. It publishes two

journals which barely provide for its present scientific output. It has also published the first volume of a contemplated series of mathematical papers. The editors, the librarian, and the secretary carry on a correspondence equal to that of a large business house. The meetings, nine each year, are largely attended, and every minute is utilized for improvement.

The society has expended from the pockets of its members, in the past eleven years, abont $15,000 for the advancement of mathematics, of which $5,000 has been spent in the last two years. We carry a balance of $2,000, of which about $900 has been reserved for a special fund ultimately for prizes or special publications, etc.; but this balance can not long be maintained.

Of all the money expended, not a cent has gone for salaries. The officers and editors all serve without the slightest pecuniary compensation; and in several cases their work is very burdensome and involves minor as well as major administration, for the officer has almost always been his own clerk, in order to avoid an expense which the society's fund did not permit. The services which the society annually receives gratis would hardly be fairly paid for by its entire income, if one may compute them in money at all.

Our membership is now 400. Our income last year from members' dues was $1,188; from sales of publications, etc., $859; from ten universities in support of the transactions, $1,000; total income, $3,747; expenditure, $3,772. We need $1,500 a year more in order that the energies of the society may be turned to full account for the advancement of mathematics in America.

Considering the representative character of the society, the skill with which it has been administered, the unselfish devotion of its administration corps, and its knowledge of needs and how to meet them, I think that the Trustees of the Carnegie fund might well feel that they could grant to the society a lump sum from year to year, to be employed as the Council of the Society might determine, accounts of course being rendered to the Trustees, who would control the situation perfectly, since they could refuse further appropriation at the end of any year. My point is that the Mathematical Society is in a position to administer any fund granted to it, in the best possible way, and its history is evidence that it will not fail to do so.

But if it is decided to make specific appropriation, then I would submit the following synopsis of the society's various activities which would be promoted by subvention:

First. The society's publications, viz., the *Bulletin* and the *Transactions*, with possibly an occasional separate publication. All these can turn money to good account. Some enlargement is already desirable, in view of the increasing number of valuable articles offered for publication.

Speaking for the *Bulletin*, I must say that the expense of editing, which was last year $33.94, can not longer be kept down to any such vanishing point. The editors can not afford to attend to all the mechanical details at the expense of the greater interests of the *Bulletin*.

Second. The library of the society is now growing rapidly. It is becoming a credit to the society, and, like all healthy institutions, is in need of money. The files should be extended and the back numbers purchased when they can not be got by exchange. We need $100 a year for this purpose, and can use $500.

Third. We have had three successive colloquia in connection with our summer meetings. These are courses of lectures by specialists, and tend most pronouncedly to the early dissemination among the mathematicians of the country of important mathematical advances and discoveries. The lecturers have received only a trifling honorarium. For the promotion of mathematics at the very top, nothing could be more effective than the foundation of one or two lectureships, to be held for one year, by incumbents who are able to treat the very latest phases of some branch of mathematics, the lecture to be arranged as heretofore at the colloquia. This would involve an expense of, say, $100 to $200 a year—*i. e.*, $200 to $400 every alternate year.

Fourth. In order that administrative officers may attend to pressing matters of larger policy, a moderate amount should be available for clerk hire. The librarian and the secretary can not much longer look after the multitude of petty mechanical details which the administration of so large and active a society as ours involves. Mr. Carnegie did not conduct his business on any such plan.

Fifth. Those specialists who pass on the suitability of papers for publication, particularly in the *Transactions*, should receive a reasonable compensation, or, rather, *honorarium*, for it need not be any large amount. It will not be always possible to receive satisfactory expert advice gratis, and this very advice is what must decide the character, good or bad, of the publishing journal. I regard a moderate expenditure in this direction as a good business and scientific policy.

There are other interests, and some interests vary and alternate from year to year. As I have said, I regard the society itself as the best judge of their merits, and I would much prefer that it should be the distributing medium. It would be fortunate, for example, if any one interest should be so preferred by the Carnegie Trustees that the judgment of the Council should not approve of the conditions. In my opinion, the Council should appoint a committee to look over the field and report, and should then present its views to your Committee for mutual discussion, the conclusion being in the form of a recommendation by your Committee to the Carnegie Trustees.

The above is a general expression of my personal views. I have endeavored to be brief, and possibly I should have recited some matters more at length; but as you are a member of the Committee of Publication and familiar with the affairs of the society, the above will probably suffice to suggest the details which I have omitted.

Sincerely yours,

F. N. COLE.

APPENDIX B

PROPOSED EXPLORATIONS AND INVESTIGATIONS ON A LARGE SCALE

CONTENTS

INTRODUCTION.

Among the applications and projects received by the Institution were several of large scope that have not been mentioned in the reports of the Advisory Committees, printed in Appendix A. There are others not yet considered that may be referred to in a future report. The Advisory Committees reported them to be of great interest and importance, but deemed it unnecessary to say more until the plans and policy of the Institution had been further developed and determined. Nine of the more important projects are given here.

PLAN FOR A BIOLOGICAL SURVEY OF THE PALEARCTIC REGION

By Leonhard Stejneger and Gerrit S. Miller, Jr.

CONTENTS

WASHINGTON, D. C., *May 19, 1902.*

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: In response to the invitation of the Carnegie Institution through its circular received in March, 1902, the undersigned would respectfully submit for the consideration of the Executive Committee the following plan for a Biological Survey of that portion of the Old World lying north of the tropics, and technically known as the Palearctic Region. An outline of the magnitude, variety, and importance of the biological problems on which this work would bear is presented in the pages which follow. To this is added a detailed plan for the organization of the work, together with an estimate of its cost. This enterprise, if undertaken, would bear a close supplemental relationship to the similar work which has been carried on with such eminent success during the past fifteen years in America under the auspices of the United States Government, and would furthermore be the direct continuation of studies which have largely occupied the attention of one of the writers for eight years and of the other for more than three times that period.

Very respectfully,

LEONHARD STEJNEGER.
GERRIT S. MILLER, JR.

I. INTRODUCTION.

1. *Nature of Work Proposed.*—Briefly stated, the proposed investigations consist in the thorough exploration of the northern portion of the Old World by parties of competent, trained field naturalists, with the special objects of determining in detail the character of the fauna and flora, the number and geographic extent of the life areas, the history of the origin of the present assemblages of animals and plants, and, finally, the relationship of the present life of the region to that of northern America. That these large, fundamental problems are now vaguely or not at all understood will be shown in the special section of this report devoted to them.

As the material for the elucidation of these problems does not exist in any museums or museum, a very important part of the work would be the bringing together of specimens collected by the field parties, and their study at home by specialists. That the extent and value of these collections would be unprecedented there can be no doubt. It would be proposed to make as complete collections as possible of the mammals, birds, reptiles, batrachians, and fresh water

fishes, together with such insects, mollusks, and plants as may be found to have special bearing on the work. It is hardly necessary to add that extensive publications would undoubtedly result from the elaboration of the collections.

As to the length of time required for this work, it is difficult to make any close estimate. If carried out on the scale here proposed, the field explorations would probably be fairly complete in ten years.

2. *Reasons Why it Should Be Undertaken by the Carnegie Institution.*—For generations zoölogists and botanists have been working piecemeal at these problems, and have expended a vast amount of labor and learning, yet it is a deplorable fact that no adequate results have been achieved. It would carry us too far were we to discuss the reasons why the Old World workers have been unable to gather the necessary material and work it up in a harmonious, comprehensive manner, so as to make the results available to others for purposes of coördination and generalization. They are of a historical, political, and financial nature. In the meantime our own countrymen have advanced so far in these particular respects—thanks to well planned, well executed, and comprehensive work by the U. S. Biological Survey—that the inaccessibility of the corresponding facts relating to the fauna and flora of the Old World has become a formidable obstacle to our own progress.

Unfortunately, the conditions which in the Old World have contributed to bring about this situation are permanent, so that no relief can be hoped for from that direction. Moreover, no Old World museum, university, or academy could undertake the work, even if so inclined and with the money, because it would be practically impossible for them to duplicate the work already so thoroughly done on this hemisphere. If done at all, the work must be undertaken from this side. We are already far ahead in the survey of our field, and the Old World material must be gathered by us, with our methods to make it comparable, worked up by our scientists to insure its uniform elaboration, and deposited in our institutions to accomplish a really valuable correlation; but thus far we have labored under the same difficulties as the Old World biologists. No institution in our country has been so situated scientifically and financially that it could think of realizing so comprehensive a scheme; nor can the Government be expected to extend its activities into this field on a scale which would promise results within a measurable future.

Other problems, such as the investigation of the more truly American fauna and flora to the south of us, as well as the biolog-

ical exploration of the vast territories recently acquired by the United States, are more likely to occupy the attention and energies of our governmental surveys.

Under these circumstances the Carnegie Institution, untrammeled by political boundaries and in possession of sufficient means, seems to be the proper agency for carrying out this important enterprise.

In brief, our contention may be summed as follows:

The proposed scheme would be of great value to biologic science.

The need for undertaking the work now is urgent.

The plan is practicable; it is no experiment; its promoters have largely devoted themselves to it; the methods are perfected; the time is ripe; the cost is moderate; no other institution can now undertake it.

The Carnegie Institution must therefore take it up if the important scientific questions involved are to be satisfactorily solved in the present generation.

3. *Feasibility of the Work.*—Geological exploration on a large scale and with a definite object in view has long been recognized as a profitable scientific enterprise. The importance of the results achieved by such investigations are so well known that they need no more than this passing allusion. In the same way extensive, specially equipped expeditions for investigating the life of the deep sea bottom are almost continually at work; vast sums are annually spent on their equipment and maintenance, and the value of the information thus obtained amply justifies the expenditure. In our special field precedent is not so widely known, and yet it has been fully established. During the past fifteen years the United States Department of Agriculture has been conducting a biological survey of North America, upon the lines of which, as elaborated by Dr. C. Hart Merriam, we would propose to establish our work in the Old World. Through Dr. Merriam's energy and experience the technique of exploration of this kind has been perfected in the most minute detail as regards both the collecting of specimens and, what is even more important, the taking of proper observations. This work has resulted in an entirely new conception of the geographic distribution of the life of North America, to say nothing of a vast increase in our knowledge of the fauna and flora of the entire region. So successful has this work been, and so economically conducted, that biological exploration on land, on a large scale, can no longer be regarded as an experiment. By arranging the work in accordance with these tested methods, with

which both of the writers have become well acquainted, the Carnegie Institution would be spared all danger of risking support to an untried venture or in experimenting with partly understood methods.

4. *Impossibility of Carrying on the Work in Any Other Manner.*—Though previously alluded to, it may be well to state more explicitly the reason why this work, if done at all, must be carried on in the manner here proposed. It might be supposed that the material required for this investigation can be readily secured by exchange or purchase, or perhaps that it might be examined by visiting different museums. But such is not the case. The material does not exist, and, strange though it may appear, it would be no more impracticable to attempt the study of the fauna of the remotest sea bottom without the aid of specially equipped expeditions than to undertake the revision of the European members of a genus of mammals or birds without first procuring material in the same manner; and what is true of such relatively small questions is equally so of the broader problems on which the proposed work would bear. If all the data now existing in museums were brought together it would be an insignificant fragment of the mass required.

5. *Qualifications of the Writers.*—The qualifications of the writers for carrying on the proposed survey are perhaps best indicated by the appended bibliographies of papers published by them dealing with the problems under consideration.

Papers by Leonhard Stejneger Bearing on the Subjects to be Investigated.

The following list is limited to the more important papers dealing exclusively with the Palearctic biota. Papers of a more general character, such as descriptions of the countries visited and of the exploration trips undertaken in the region treated of, are not included.

1871. Ornithologische Notizen aus Meran, Sud-Tirol. Journ. f. Orn., 1871, pp. 122–124. "Nachtrag," tom. cit-, pp. 462, 463.

1873. Ornithologisches aus Norwegen, I and II. Journ. f. Orn., 1873, pp. 304–307.

1873. Norsk Ornitologisk Ekskursjons Fauna (Christiania), 8vo. XVI and 111 pp. and 4 lith. pl. (A pocket manual of Norwegian birds.)

1874. Norsk Mastozoologisk Ekskursjons Fauna (Christiania), 8vo. VIII and 31 pp. (A pocket manual of Norwegian mammals.)

1875. Ornithologisches aus Norwegen, III. Journ. f. Orn., 1875, p. 167.

1878. Ornithologisches aus Norwegen (IV). Ornith. Centralbl., 1878, p. 109.

1878. Underslgæten Lanius. Arch. f. Mathem. Naturvid., 1878, pp. 323–339. (A monograph of the subgenus Lanius.)
1879 Bidrag til Vestlandets ornithologiske Fauna. Nyt. Mag. Naturvid., 1879, pp. 141–148. (Contributions to the ornithology of western Norway.)
1882. Andet Bidrag til Vestlandets ornithologiske Fauna. Nyt. Mag. Naturvid., 1882, pp. 111–124. (Contributions to the ornithology of western Norway, second installment.)
1882. Outlines of a Monograph of the Cygninæ. Proc. U. S. Nat. Mus., V, 1882, pp. 174–221. (The nearctic and palæarctic swans treated of in detail.)
1883. Contributions to the History of the Commander Islands No. 1. Notes on the Natural History, including descriptions of New Cetaceans. Proc. U. S. Nat. Mus., VI, 1883, pp. 58–89.
1884. A brief Review of the Lagopodes belonging to the group Attagen. Zeitsch. f. ges. Ornith., I, pp. 86–92 and pl. V. (Monographic essay on the Ptarmigans.)
1884. Pseudototanus guttifer (Nordm). Zeitsch. f. Ges. Ornith., I, pp. 223–229 and pl. X. (Rediscovery in Kamchatka of this bird, which had been lost for fifty years.)
1884. Remarks on the Species of the Genus Cepphus. Proc. U. S. Nat. Mus., VII, pp. 210–229. (A monograph of this holarctic genus.)
1884. Analecta Ornithologica. I. The Occurrence of Turdus aliciæ in the Palæarctic Region. XI. Notes on Arctic Lari. Auk, 1884, pp. 166, 360–362.
1884. Diagnosis of New Species of Birds from Kamchatka and the Commander Islands. Proc. Biol. Soc. Washington, 1884, pp. 97, 98. (Description of five new species collected by the author in Kamchatka.)
1884. Investigations Relating to the date of the Extermination of Steller's Sea-Cow. Proc. U. S. Nat. Mus., VII, pp. 181–189. (No. 2 of the Contributions to the History of the Commander Islands.)
1884. Additional Notes on the Plants of the Commander Islands. Proc. U. S. Nat. Mus., VII, pp. 529–538. (No. 4 B of the Contributions to the History of the Commander Islands.)
1884. A New Species of Woodpecker from Kamchatka. Auk, I, pp. 35, 36.
1884. Notes on the Genus Acanthis Auk, I, pp. 145–156. (A monographic paper on this holarctic genus.)
1884. Ueber einige Formen der Untergattung Anorthura. Zeitschr. Ges. Orn., I, pp. 6–14. (Monographic; holarctic.)
1885. Results of Ornithological Explorations in the Commander Islands and in Kamchatka. Bull. No. 29, U. S. Nat. Mus., 382 pp. and 9 plates.
1886. The British Marsh-Tit. Proc. U. S. Nat. Mus, IX, 1886, pp. 200, 201. (Described as new.)
1886. Review of Japanese Birds. I. The Woodpeckers. Proc. U. S. Nat. Mus., IX, pp. 99–124.
1886. On Brachyrhamphus perdix (Pall.) and its nearest allies. Zeitsch. f. Ges. Ornith., III, pp. 210–219 and pl. VII. (Monographic; North Pacific.)
1887. Description of a New Species of Fruit-Pigeon from Liu Kiu Islands, Japan. Amer. Natural., XXI, pp. 583, 584.
1887. Lundefuglene i det Stille Hav. Naturen, 1887, pp. 33–38. (The Puffins of the North Pacific Ocean.)

1887. On a Collection of Birds Made in the Liu Kiu Islands, with Descriptions of New Species. Proc. U. S. Nat. Mus., IX, 1886, pp. 634–651.
1887. Review of Japanese Birds. II. Tits and Nuthatches. Proc. U. S. Nat. Mus., IX, 1886, pp. 374–394.
1887. Review of Japanese Birds. III. Rails, Gallinules, and Coots. Proc. U. S. Nat. Mus., IX, 1886, pp. 395–408.
1887. Review of Japanese Birds. IV. Synopsis of the Genus Turdus. Proc. U. S. Nat. Mus., X, 1887, pp. 4, 5.
1887. Review of Japanese Birds. V. Ibises, Storks, and Herons. Proc. U. S. Nat. Mus., X, 1887, pp. 271–319 and pl. X.
1887. Review of Japanese Birds. VI. The Pigeons. Proc. U. S. Nat. Mus., X, 1887, pp. 416–429 and pl. XXII.
1887. On the Extermination of the Great Northern Sea-Cow (Rytina). Bull. Amer. Geog. Soc., 1886, pp. 317–328.
1887. Further Contributions to the Avifauna of the Liu Kiu Islands, Japan, with Descriptions of New Species. Proc. U. S. Nat. Mus., X, 1887, pp. 391–415 and pls. XXI, XXII.
1887. On Turdus alpestris and Turdus torquatus, two distinct species of European Thrushes. Proc. U. S. Nat. Mus., IX, 1886, pp. 365–373.
1887. Diagnosis of a New Species of Thrush (Turdus celænops) from Japan. Science, X, 1887, p. 108.
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1887. Notes on the Northern Palæarctic Bullfinches. Proc. U. S. Nat. Mus., X, pp. 103–110. (Monographic.)
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II. Problems.

1. *Relationship Between Life of Northern America and Northern Europe and Asia.*—With regard to the relationship of the life of the northern portion of the Old World to that of northern America there exists the greatest diversity of opinion. Certain writers hold that the life of each of the two great northern land masses shows enough peculiarities of its own to warrant the recognition of a separate *Nearctic region*, and *Palearctic region* of primary rank, each, therefore, coördinate with such biogeographic areas as the *Australian region*, *Ethiopian region*, etc. Others equally competent to interpret the facts regard the biota of the two areas as practically identical, so that only when united to form a *Holarctic region* do they become strictly comparable with the other primary geographic divisions of the earth's life. In general European writers favor the former view, while Americans unanimously support the latter. That such radical difference of opinion can occur is mainly due to the fact that each party bases its assumption on confessedly inadequate data, due to the total lack of such definite information concerning the life of Europe and Asia as is now available for northern America. As an instance of the uncertainty which exists with regard to the actual relationship of the life of the *Palearctic* and *Nearctic regions*, the flowering plants, among the most carefully collected and studied of organisms, may be cited. In the latest edition of Gray's Manual about 425 species are given as common to the northeastern United States and the *Palearctic* region. Since the publication of this work many of these have been critically studied and found to be in reality aggregates of numerous closely related forms, no one of which occurs on both sides of the Atlantic. The genera *Agrimonia*, *Linnæa*, and *Antennaria* are conspicuous examples. As yet, however, no systematic general study of these species has been undertaken, so that it is impossible to say what conclusions in regard to the relationship of the life of the Old World to that of America may be drawn from the apparent similarity in this large number of species.

2. *Life Areas of Northern Europe and Asia.*—Equal divergence of opinion exists on the question of the manner of distribution of life within the two great land areas. The fact is generally admitted by American writers that the life of the so-called Nearctic region is arranged in zones, or belts of relative homogeneity, bounded by certain isothermal lines, and, like the isotherms, extending in a general

east and west direction, but subject to deflection northward by low, hot plains, and southward by mountain ranges; also that in the ascent of a mountain rising from the area occupied by one of the more southerly life zones, assemblages of animals and plants closely corresponding with those of the more northerly zones are successively encountered in regions whose altitude produces the requisite lower temperature. By nearly all students of the *Palearctic* region, on the other hand, it is supposed that the life areas of Europe and Asia form large blocks, of no definite form and bearing no close relation to isothermal boundaries. Thus, there is commonly recognized a *European* subregion extending from the barren shores of the Arctic Ocean to the luxuriant coast of the Mediterranean, and embracing the great mountain mass of the Alps. This subregion is by some writers contrasted with a *Siberian* subregion, considered as of equal rank; by others united with it to form a *Europasian* subregion. Other divisions of the *Palearctic* region are the *Manchurian* subregion, the *Mediterranean* sub-region, the *Eremian* subregion, etc. In all this there is the greatest diversity of opinion, no two writers agreeing as to the number, names, or boundaries of the subdivisions. In only one feature is there uniformity, in the absence of that universal conception of zonal arrangement of the life areas that characterizes the work of American writers on the zoögeography of the *Nearctic* region. In fact, one European author has recently gone so far as to point out reasons why zonal distribution can not exist in certain portions of the Old World.

But is it probable that such a radical difference in the distribution of the life of America and Asia actually exists? Does one law of distribution hold good in one continent and a wholly different law in the other, even though many of the animals and plants of the two regions are by all writers admitted to be closely related? Or are the life zones of America represented by strictly homologous areas in the Old World, dependent on the same underlying physiological laws? These questions are perhaps the most important now before the student of the general dispersion of life in the northern hemisphere.

Their answer was hinted at by several writers of half a century ago, whose work is now generally overlooked. Agassiz, in 1850, went so far as not only to recognize the zonal distribution of life in the Alps and the region lying to the north of them, but also to suggest the correlation of the life zones which he had observed in Europe with those of eastern North America. One of the present writers

has spent much time in the attempt to follow out this course of investigation, but beyond the discovery of much scattered evidence no result has been possible, owing to the absence of material. He has been able, however, to construct a preliminary map of the life zones of Europe, and his success, with the very limited data at his disposal, points toward a speedy and logical settlement of the question as a certain result of properly conducted explorations.

3. *Former Glaciation and Its Bearing on the Present Distribution of Life.*—It is scarcely necessary to allude to the close connection between the more directly zoölogical questions and those of Post-Tertiary paleontology and geology, but we may call attention to the fact that recently attempts have been made, on biological grounds, to belittle the extent and intensity of the glacial phenomena in Europe during the period known as the Ice Age. It has been argued, with considerable plausibility, that the conditions in Europe at that time were such that a universal glaciation must have been an impossibility, and that, so far from being colder, the climate of Europe was then milder than at present. In our opinion, such theories have no actual foundations, and we believe that it is only defective knowledge of the minute details of the fauna and flora of the entire northern world that is responsible for the fact that they can be propounded and defended, a view publicly expressed not long ago by one of the present writers. Nevertheless, it can not be denied that with the present status of our knowledge the discussion might go on indefinitely. A better knowledge of the life zones of the Old World would probably also greatly promote the correct understanding of the biological side, at least, of the interglacial question.

If the climate of Europe in the glacial period is the source of much contention between widely diverging views, that of Siberia and of the Asiatic Pacific coast is still more in doubt. While nobody may be found to advocate a glaciation equaling in extent, intensity, and duration that of Europe or North America, there is no lack of authorities who maintain that the climate must have been quite severe and that glaciers were not absent. On the other hand, an author has within the last two years defended the proposition that the climate of that region did not differ materially from that of today.

4. *Manner of Expansion of Faunas.*—It has been supposed that a large proportion of the present fauna of Europe on the one hand

and of America on the other had its origin somewhere in central Asia, having spread fan-like in both an easterly and westerly direction. From various paleontologic as well as biologic facts of a more or less fragmentary nature, it is generally held that such invasions took place during glacial times, both before and after the maximum glaciation was reached. But there are clear indications that such expansion from Asia to Europe and to America is going on today. In a paper read before the Biological Society of Washington, one of the present writers was able to demonstrate this fact with regard to a limited number of species of mammals and birds in northern Russia which in their westward march are nearing the Atlantic or have reached it. The data for northeastern Siberia and Alaska are still more fragmentary, yet there are indications pointing to a similar eastward movement of the Siberian animals. It will be easily seen how important it is to determine as far as possible the extent and volume of this invasion.

While it would seem from a contemplation of the cases known that this expansion is made gradually by the species extending its range at the periphery of its distribution, it can not be denied that some animals may have spread by means of sudden irruption of great numbers into hitherto unoccupied territory. As examples of such explosive movements we may cite the occasional invasion of Europe by great numbers of the Asiatic sand grouse, as in 1863 and 1888; similar, though more frequent irruptions of the spotted nutcracker; and the well known wanderings of various rodents, such as the lemmings. A full study of these phenomena may have a very important bearing upon the question of the proper interpretation of many finds of fossils. If a future paleontologist were to find in England the bones of the sand grouse in some deposit from 1863 he might conclude that that species had been native of Great Britain, and that consequently a climate like that of the central Asiatic steppes prevailed in England in 1863. As the lemming during its periodical wanderings may deposit its bones far beyond the limits of its proper life zone, it is plain that the discovery of its fossil or subfossil remains is not in itself sufficient proof of the former existence of that life zone at the locality in question.

5. *Relation of the Cave Fauna of Europe to Existing Life.*—It is well known that caverns in various parts of central and southern Europe contain the bones of animals of a kind very different from those now inhabiting the region. In many instances these repre-

sent extinct species; but it frequently happens that the remains are referred to members of the fauna now existing in other regions. In the majority of such cases the animals are now found far to the northward, the unavoidable conclusion being that when the cave deposits were formed the climate was such as to permit their existence in the neighborhood, yet in few instances has sufficient critical comparison been made between the cave remains and the bones of the existing species they are supposed to represent. Hence it is impossible to determine whether the time which has elapsed since the climate of central Europe was such as to allow the existence of arctic animals is sufficiently great to have permitted the survivors to become in any way differentiated, or whether it is so short that no changes have taken place. In one instance—the lemming of the cave in Portugal—in which a comparison of this kind was made, the result showed that the remains differ materially from those of the animal now living in Norway. To continue such comparisons would be an important branch of the investigations here proposed.

6. *Migration of Birds in Northern Europe and Asia.*—Palmén has clearly demonstrated that the routes of certain migrating birds furnish good indications of former land connections, or former shore lines, as the case may be, as well as of the road by which the species have extended their range from the original center of distribution. For the solution of the problems before us it is therefore of the utmost importance that these migration routes be determined and analyzed in detail for as many species as possible. Palmén himself was able to trace several such routes in Europe and western Asia which have shed considerable light on former distribution of land and water. I need only refer to the route from Novaya Zemlya to the White Sea, to the Gulf of Finland along the Baltic, across Holstein, to the North Sea and southern England and Ireland, thus plainly outlining the shore of the Arctic marine transgression at a certain period in glacial times. Similar routes across the Mediterranean have been shown to follow the old land bridges between Europe and Africa. In Europe these routes have been indicated for a number of species, though many birds are simply referred to them on general principles. In Asia, however, the work of determining these routes is still in its hypothetic stage. One of the present writers as far back as 1885 broadly outlined the migration route of at least two species from Bering Strait to India, showing it to lie in an entirely

unsuspected direction through the interior of the continent, and he also indicated the routes of other species in a tentative way.

Pressure of other work, as well as lack of material, has prevented further research in that direction, and no one else has taken up the subject or is liable to do so. A year ago, however, he was led to investigate the routes of an Old World migrating bird which extends its range into Greenland and eastern North America on the one side and into Alaska on the other. He was able to prove that the bird in these two localities is represented by two subspecies, one of which migrates in winter to western Africa, the other to India. The routes thus traced clearly indicated the way by which the species originally invaded America. To better illustrate the importance of the investigation we may be permitted to quote the concluding paragraph of the generalization based upon it, as follows:

> "It seems that one more lesson can fairly be drawn from the differentiation of the Greenland race, viz., that the Greenland-Iceland-England route must be considerably older than the Alaska-Chukchi-Udski route, since it has resulted in the establishment of a separable race. A consideration of the further fact that no regular migration route could have been effected between Greenland, Iceland, and Great Britain during the present distribution of land and water in that part of the world also leads us back to a period when the stretches of ocean now separating those islands were more or less bridged over by land. For such a condition of affairs we shall have to look toward the beginning of the Glacial period. At that time it must, therefore, be assumed that the Wheatear extended its range into Greenland. The advent of the typical form into Alaska, on the other hand, is probably one of very recent time, an assumption corroborated by the somewhat uncertain and erratic distribution of the species in that northwestern corner of our continent." (Proc. U. S. Nat. Mus., XXIII, 1901, p. 477.)

The exceptionally satisfactory results of this investigation were due to the coincidence of a fairly good series of specimens in the National Museum with the fact that the difference between the two races was one of length of wing, and that consequently the measurements by European authors could be utilized. If the distinguishing characters had been of a different nature, the material at hand would have been insufficient.

It will thus be easily seen that if we had the material we could determine these routes for many species, and thus secure valuable facts for a correct interpretation of many of the most important problems now under discussion.

7. *Origin of the Present Fauna of Europe.*—Considerable light has of late years been thrown on the question of the origin of the European fauna; yet while certain general propositions may be said to have gained common acceptance because based on fairly convincing evidence, many others are based chiefly upon more or less plausible theories, for which the proof in the shape of sufficient material is still lacking. On the other hand, there are numerous important questions where the opinions and theories of those best equipped to judge are diametrically opposite, not because the nature of the questions is such as to preclude an indisputable settlement, but simply because there is not material in any one museum, nor even in all museums, adequate to establish the status of the species or their geographical distribution.

It would be impracticable in the present connection to go into detail with regard to all these questions or to prove the above assertion for the various classes of animals. It must suffice to mention a few flagrant cases which may stand as examples of the rest.

One of the most important problems, as far as the origin of the European fauna is concerned, is the question whence came the animals which Doctor Scharff has termed the "Arctic migration." He, with many others, contends that until toward the end of the glacial period there existed a continuous land connection between America and Europe, far north between Greenland, Spitzbergen, and Scandinavia, the latter being again connected by a land bridge with Scotland across the North Sea, and England with France. Across this continuous land bridge these animals are supposed by him to have wandered into central Europe. One of the writers has tried to disprove this hypothesis, both on physiographical and biological grounds, and has advanced the theory that these animals reached central Europe and Great Britain from western Siberia before the maximum glaciation, and that from Scotland they extended into western Norway. Unfortunately, however, there is nowhere any material gathered together which would help settle the question.

It is unnecessary to here detail the gaps in our knowledge which make a discussion of this question a mere guessing contest. Suffice it to say that some of the most elementary facts concerning the relations and distribution of some of the largest, most conspicuous, and most important species are in dispute. Indispensable details as to the distribution and relationships of the ermine, the lemmings, the deer, the reindeer, the grouse, the ptarmigan, etc., are still lacking and, except by some lucky chance, may not be forthcoming for a long

time. Vague notions abound but there is little certainty. And if our knowledge of the big and conspicuous animals is so defective, what may be expected of the obscure and rare forms?

We have selected the above question as an illustration because it has so direct a bearing upon the history of our own North American fauna; for clearly, if such a continuous land bridge existed between the Old and the New Worlds toward the early days of the glacial period that reindeer and ermine and lemmings could pass from America to Europe, we are justified in inquiring into the question which animals wandered from Europe into America by the same bridge.

But while we do not deem it necessary to go into further detail, it may be mentioned, as highly instructive in this connection, that notwithstanding all that has been written as to the relationship of the animals in the Scandinavian mountains and the Alps, and in spite of all the theories based upon this fact, such as that of the gradual following up of the receding northern and southern glaciers by the arctic inhabitants of the central European plains, no satisfactory comparison in the modern sense of the animals most important in this respect has ever been undertaken. It is a fact worth noting that it is little more than a year since one of the present writers distinguished between the Scandinavian and central European varying hares. This separation made here in America, from material in American museums, emphasizes the need of the work here proposed.

8. *Special problems in Eastern Asia.*

(*a*) *Former land connection between Asia and America.*—Turning now to the eastern or Pacific side of the Eurasian continent, we find equally important problems in even less satisfactory condition. As far as they have a bearing on North American problems, it may be said that their importance to us is even greater than those we have mentioned above. Whatever we may think of a land connection in glacial times between America and the Old World by way of Spitzbergen or Iceland, there is less doubt as to the connection by way of Bering strait, and while it is doubtful whether America has received any additions to its Pleistocene or Postpleistocene fauna or flora by way of the former, there is scarcely any doubt that a large influx came by way of the latter. While there is a general feeling among biologists that such is the case, it is very difficult to prove it in detail, as was clearly shown during a recent "symposium" or discussion of the matter in the Washington Biological

Society, during which there were adduced many general observations tending to substantiate such a claim, but very little positive evidence for or against. This is equally true of zoology, botany, paleontology, and geology. The tendency was to regard Bering strait as the probable route; yet it must not be overlooked that so prominent an authority as Dr. Henry Woodward, as lately as 1894, could state with regard to the points brought out by Dr. George M. Dawson, in a paper then under discussion, that he regarded Dawson's paper as proving that the Aleutian islands are the old high road for the mammoth and other mammals from Asia into North America in Pleistocene times (Quart. Jour. Geol. Soc., Vol. L, Feb., 1894, p. 9). In looking for the evidence of this, one is struck by the extremely meager details upon which such sweeping generalizations are based, as well as by the uncertainty of even these few facts. Thus the mainstay of Dr. Woodward's contention is a record of a few bones of mammoth having been found by "some men, probably Russian promyshleniks," on Unalaska Island in 1801.

(*b*) *Japan.*—The fauna and flora of the outlying islands and peninsulas of northeastern Asia is fully as much a key to the situation there as that of Great Britain, Scandinavia, and Spain is to similar problems in Europe. If we had the material to properly interpret the present and past distribution of life in the Chukchi peninsula, Kamchatka, the Kurils, Sakhalin, and the Japanese islands we might easily trace the history of the migrations of the land animals in that part of the world and the expansion of the Asiatic fauna and flora into the northern portion of our own continent. But no such material is at hand. The U. S. National Museum probably has the best collection of Japanese birds, and one of the present writers has devoted years to its study, and published numerous papers upon details of Japanese ornithology; but lack of means has prevented the completion of the work, for which a large amount of manuscript has been accumulated, but never published, simply because he has been unable to secure the necessary material. He has also in hand an extensive work on the herpetology of those regions, which is well under way toward completion; but it can only be regarded as a preliminary reconnaissance, the material being too fragmentary for an exhaustive treatise, though based upon the two largest collections of the kind, those of the U. S. National Museum and of the Imperial University in Tokyo. Our knowledge of the Japanese mammals is even less advanced than it was seventy years ago, and there is absolutely no material in any museum. Large por-

tions of Japan are complete *terrae incognitae* as far as the higher animals are concerned; thus, for instance, the whole large island of Shikoku. The Japanese themselves have had so many other problems to solve that their scientific activity has as yet yielded but slight results in our direction.

(*c*) *Kamchatka.*—As for the peninsula of Kamchatka, very little biological work has been done there. One of the present writers has had the good fortune to be able to collect at one place in the southern part and has published on the birds of the region. He was enabled to draw some general conclusions as to the character of the fauna, and to indicate, although in a very tentative way, the insular nature of its biota, conclusions since corroborated by an eminent geologist. But beyond these hesitating suggestions nothing definite can be asserted from lack of extensive and positive information.

(*d*) *Kuril Islands.*—The Kuril Islands, which now form the connecting link between Kamchatka and Japan, have not yet been reached by the modern biologist, except that a Japanese botanist has given an account of the flora to a great extent based on antiquated information, while one of the present writers has published a preliminary account of the birds, also based to a great extent upon old and uncontrollable records, his own visit having been too brief for any extensive collecting.

Sakhalin has been studied to some extent by Russian scientists, but the results are partly inaccessible because in the Russian language, and because the material collected is now scattered.

(*e*) *Mainland of Eastern Asia.*—The adjacent portions of the Asiatic mainland, the tracts adjoining the Okhotsk Sea, and the Amur region are among the most important localities for the elucidation of the questions we propose to solve. Fifty years ago they were the scene of the explorations of Middendorf, Schrenck, and Radde, and at that time our knowledge of those regions was ahead of that of our own great West. How different today! Biological science has taken a new start since the days of those pioneers, and excellent as their work was for that time, it is utterly antiquated and inadequate today. Nothing worth speaking of has been added, and for present purposes the work is nearly useless because of its lack of detail and the absence of sufficient material in large series to substantiate the results.

These large series of specimens from the entire circumpolar area are among the greatest desiderata of biologists dealing with these problems. There is no museum in America which has even a repre-

sentative collection consisting of single specimens of typical forms occurring across the vast continent from England to Kamchatka, and no museum in the world—nay, not even all the museums put together—could furnish such series of any large group of animals as are required by modern methods.

(*f*) *Miscellaneous allied questions.*—It would carry us too far were we to go into detail, pointing out all the important problems which wait for a solution through a study of the life of Siberia and an intelligent comparison with the corresponding area of our own continent. The evolution of the steppe and tundra animals and their spreading at periods when the steppe or the tundra life zones temporarily at least extended into other regions; the significance of the marine life, the seals, crustaceans, sponges, etc., in Lake Baikal; the question as to the efficiency of the marine transgression of the Arctic Ocean east of the Urals in establishing an effective barrier against Siberian fauna and flora expanding into Europe during glacial times, either alone or in conjunction with glaciation; the climate of the interior of Siberia during the same period and the curious mixed fauna, as shown by the fossils from the New Siberian islands and the mouth of the Yana river; the very origin of the fauna which apparently radiated from some point in that vast territory reaching as far west as the British islands and as far east as Greenland—these and many other problems are closely connected and are all waiting for further elucidation based upon the bringing together under one comprehensive system of detailed material for minute study and exact correlation.

9. *Some probable systematic results.*—One of the aspects of the proposed exploration which must not be overlooked is the great increase in the systematic knowledge of the fauna and flora of northern Asia and Europe that must result from definitely planned and executed field work. While it is impossible to form any accurate estimate of the probable results in this direction, a general idea of the outcome may easily be obtained.

Before the beginning of the work of the Biological Survey of the Department of Agriculture, the known mammal fauna of North America consisted of about 350 species. As a result of the work carried on by this Survey, the number of recognized forms is now between 1,600 and 1,700. In that portion of the Old World which we propose to explore there are now known about 600 mammals. An incresase at least as proportionately great as in America is to be expected; in other words, we may look for the discovery of nearly 2,000 mammals

now quite unknown to science. It may be mentioned that as the result of some very fragmentary explorations (part of which were directed by one of the writers) in the best known part of Europe more than 60 new mammals have been discovered during the past few years.

In other classes a very great increase of the same kind is to be expected, though probably to a less extent than in mammals. Add to this the fact that the geographic distribution of practically no widely spread species of Old World vertebrate is sufficiently well known to permit its range to be mapped, and the importance of this branch of the inquiry will be appreciated. As already pointed out, much remains to be done in the study of those plants whose range is supposed to cover parts of both hemispheres. From the results already obtained it seems highly probable that a new conception of the relationship of the members of most *Holarctic* genera would result from such work systematically conducted.

III. Method of Conducting the Work.

1. *General organization.*—The general organization proposed is as follows: The work would be conducted under the joint supervision of the two directors, each of whom would take special charge of the problems toward which his previous studies have been chiefly turned, and each paying particular attention to the region in which work has already been begun—that is, one at the extreme east, the other at the west of the vast region which it is proposed to explore. The care of the financial and administrative details of the survey would be placed in the hands of an executive officer selected with special regard to business ability. The force of field naturalists would, when fully organized, consist of from twenty to thirty observers and their assistants, specially trained for the work. In Washington a building should be provided for laboratories and temporary installment of the collections.

2. *Organization of field force.*—While the organization of the field force must to a large extent depend on circumstances which can not be foreseen, it is possible to gain a clear idea of the general course to be pursued. Men already trained in the methods would be at once placed in the more readily accessible regions, accompanied by assistants who, when sufficiently experienced, would in turn become the educators of the new men, and push on into more remote regions. In this manner an efficient force would be built up and the work carried on uniformly throughout, a consideration of the utmost importance. Among the field naturalists those who show aptitude for

such work would be expected to elaborate their results for publication on their return from the field and while yet in the employ of the survey.

3. *Organization of office force.*—At the beginning of the work the office force would be small; probably a typewriter, a laborer, and a taxidermist would be sufficient in addition to the directors. Preparation of outfits would be attended to by the field agents, aided by the taxidermist. The latter would thus be quickly trained to carry on this part of the work, and to superintend it during the remainder of the survey. Material might be expected to arrive within three or four months, and this would necessitate the services of another man to attend to cataloguing and labeling. When the full number of agents are in the field, an addition to the cataloguing force will be required; exactly how much can not be estimated, but probably two or even three men. The reception of specimens will also call for an addition to the taxidermist's force, as well as for the employment of one and later more preparators for cleaning skulls, mounting plants, etc. As soon as large mammals begin to accumulate, the services of a tanner will be required for putting the specimens in a condition of safety.

The force thus outlined will doubtless be sufficient for running the work during the first five or six years. Later, as publication of results increases in importance, and extensive work is being done on the collections, an increase will be needed. Just what this will amount to can not be foreseen, but the lessening of the amount of field work will doubtless compensate for it, so far as total expense is concerned.

4. *Relationship to other institutions.*—The activities of the force of field naturalists which it is hoped would be organized should result in the bringing together yearly of from 10,000 to 20,000 specimens of vertebrates alone, in addition to which there will be large quantities of invertebrates, plants, etc. Provided it be not the policy of the Carnegie Institution to establish a museum of its own, the disposition of this material will demand careful consideration. The plan to which we would direct the attention of the Executive Committee is that temporary quarters for the collections be established in Washington, where they may be kept for elaboration and study during the course of the survey. Eventually the collections and sets of duplicates may be distributed, at the discretion of the directors of the survey and under the approval of the Carnegie Institution, to other institutions and to specialists who may have aided in the elaboration of material.

IV. Estimates of Cost.

The following estimates can be regarded as nothing more than a tentative outline of the probable expenses. While the Biological Survey of the U. S. Department of Agriculture furnishes a reliable standard, so far as methods are concerned, its expenditures have no direct bearing on those to be expected in the present case. This is owing to the much larger area to be explored, and also to the great distance between headquarters and the field to be worked.

The estimates here presented * are believed to represent the maximum outlay that may reasonably be expected. During the first years of the work a considerable saving is to be looked for, which will allow for the accumulation of a balance to be applied to unforeseen expenses during the later phases of the enterprise.

Estimate on Basis of First and Subsequent Years.

I. Field work:	
First year	$22,800
Subsequent years, per year	37,050
II. Administration and care of collections:	
First year	22,200
Subsequent years, per year	22,650
III. Contingent:	
First year	5,000
Subsequent years, per year	5,300
Total:	
First year	50,000
Subsequent years, per year	65,000

Estimates on Basis of One, Two, and Three Years.

I. Field work:	
First year	$16,150
Second year	22,300
Third and subsequent years	36,550
II. Administration and care of collections:	
First year	16,570
Second year	18,170
Third and subsequent years	21,640
III. Contingent:	
First year	2,280
Second year	4,530
Third and subsequent years	6,810
Total:	
First year	35,000
Second year	45,000
Third and subsequent years	65,000

* Details omitted in printing.

V. Opinions on the Plan.

We here subjoin the opinions of a few well known naturalists on the work proposed in the foregoing plan.

[*Dr. J. A. Allen, American Museum Natural History, N. Y., to Dr. Stejneger.*]

New York City, *October* 19, 1902.

My Dear Dr. Stejneger:

I have read with great care and interest your letter of October 13, outlining the plan and purposes of a biological survey of the Palearctic region, as proposed by you and Mr. Gerrit S. Miller, Jr., for the consideration of the Trustees of the Carnegie Institution. I am deeply impressed with its magnitude and importance. It seems like a project so far reaching and beneficent in its influence upon the progress of science that its accomplishment is almost beyond hope, even in these days of great undertakings. Such an enterprise is of course beyond the means of any individual, or of any of our museums or universities. Unless it can be taken up and carried forward as a part of the work of the Carnegie Institution, we shall have to depend in the future, as in the past, upon the independent and unco-ordinated efforts of individual explorers and collectors for any advance in our knowledge of the biota of this great region and its relation to that of North America. Collections formed in this way are in the first place unsystematic, usually limited to a few classes of objects and to a few restricted areas, and are worked up separately by specialists limited to small amounts of material and without opportunities for its proper comparison with allied material collected previously at other times and places. Under such unfavorable conditions some progress has been made, but under such methods generations will pass before any very thorough and comprehensive knowledge is gained in regard to the constitution and relationships of the biota of the different faunal and floral areas of the Holarctic region.

What is needed is a thoroughly organized and comprehensive scheme of exploration, carefully planned and systematically organized as respects field parties and the areas to be covered by them; a main base for the outfitting for field work, and to which the collections should be returned, and thence distributed to experts for elaboration. It is evident that no government will or can undertake such a survey for obvious geographical and political reasons, and

that no museum or other institution of learning can assume the financial responsibility. The field is thus clear for the work you have outlined, and unless financial aid can be supplied by the Carnegie Institution for this well conceived undertaking it will doubtless remain merely the dream of its projectors.

The biota of the Palearctic region is at present only very imperfectly known, and very little material, comparatively speaking, has been collected, and that only in a casual and superficial way. Moreover, this is widely scattered and practically inaccessible to the students of any special group or area. Hence its bearing on general problems, as to former land connections between Eurasia and North America, the place of origin of groups common to the two continents, the relation of particular phases or aspects of plant and animal life to their special environment, can not be determined. Under your plan of consecutive and systematic field work for a period of years, under the most careful instructions as to methods of preparation, biologists would soon be in possession of means for the determination of all these questions, resulting in a most marked advance in knowledge along these lines, which could not fail to have also a direct and powerful influence upon the general progress of biology. The amount of material thus gathered in accordance with modern American methods of field work could not fail to be very great, and after elaboration would be available for distribution among a large number of scientific institutions, where it would have permanent value as a part of the records of science and as a basis of future comparative research. Its elaboration would give opportunity for some of the younger investigators, to whom some of the material would naturally be submitted, to show their ability for research, and thus promote one of the primary purposes of the Carnegie gift, the discovery of talent and favorable opportunity for its development.

Our United States Biological Survey, as conducted by Dr. Merriam, is an illustration of what may be accomplished by the use of moderate means expended under wise direction. You and Mr. Miller have been more or less associated with this work, and are familiar with the most approved modern methods of field work. I am sure also that you are conversant with all the details of an effective inception of the great undertaking you have so intelligently proposed, and sufficiently in touch with all the leaders in biological investigation to be able to give wise suggestions as to the apportionment of the material for elaboration among those best fitted for such work.

Your proposal for a biological survey of the great Palearctic region has thus my most earnest endorsement, and my best wishes for its favorable consideration by the administrators of the Carnegie fund, a portion of the income from which, it seems to me, may most fittingly be devoted to such work.

Sincerely yours, J. A. ALLEN.

[*Dr. Theo. Gill, Smithsonian Institution, to Dr. Stejneger.*]

WASHINGTON, D. C., *October* 14, 1902.

DEAR DR. STEJNEGER:

I have carefully read the copy of your and Mr. Miller's detailed proposition to the Carnegie Institution for the biological exploration of the Palearctic region, and the more I consider it the more favorably it impresses me. I presume it will be a revelation to most of our friends that the zoogeography of Europe is in need of further elucidation, and yet all your statements are perfectly true. Not only is the geographic distribution of life in the Palearctic region very imperfectly understood, but also in every department of zoology with which I am familiar uncertainty prevails respecting the species and the value of differential characters. Such questions, all of primary importance in biology, can only be settled by systematic exploration of the kind you have planned. You make especial mention of the mammal, bird, and reptile fauna, and I can bear witness to the fact that the fishes and mollusks are enveloped in a similar cloud of doubt. No two observers have independently reached the same conclusion as to the systematic relation of any large and varied group.

I hope you will be successful in obtaining the aid of the Carnegie Institution in undertaking the proposed exploration. It will interfere with no work now being done, and no other institution can possibly undertake it. Such an "American invasion" will be of benefit to all parties.

Very truly yours, THEO. GILL.

[*Dr. David Starr Jordan, President Stanford University, to Dr. Stejneger.*

STANFORD UNIVERSITY, CAL., *October* 17, 1902.

DR. LEONHARD STEJNEGER,

Smithsonian Institution, Washington, D. C.

DEAR SIR: I am deeply interested in your project for a natural history survey of northern Asia. As you know, my own work in the

Pacific is largely connected with such a survey, and until the fauna of eastern Asia is well known all our work on the fauna of the western Pacific must be tentative. The fauna and flora of western North America and of the Pacific Ocean are largely derived from Asiatic sources, and until we have studied Asia we are in the position in which students of American history would be if the history and peoples of Europe were practically unknown. The parts of biology which deal with evolution, with geographical distribution, and with relations to environment would be immeasurably benefited by such a survey as you contemplate, and I sincerely hope that the Carnegie Institution may see fit to provide for it.

Under present auspices, it will be wholly impossible to do this work within a century. The small funds the American universities can spare go but a little way, and so far as the European institutions are concerned, all that has been done is of the slightest importance in proportion to the magnitude of the problems.

There could, therefore, be no interference with the work of others. In fact, all interested in these problems would derive their best material from this survey, and their energies would thus be enlisted in it.

The plan as detailed to me seems to me entirely practicable at a moderate expenditure each year for a number of years.

I need not say that there can be no possible question as to the fitness of yourself and Mr. Miller to direct this work. American naturalists have long recognized in you one of the master minds of zoology, and you have been as successful in practical work, as that of the Fur Seal Commission, as in the research work of pure science.

Not one of the various projects submitted to the Trustees of the Carnegie Institution interests me personally so much as yours, and none can have a more far reaching relation to the development of biological science.

It is our opportunity to claim the Pacific Ocean and to study its shores and its life with the thoroughness that our colleagues in Europe have devoted for a century to the Atlantic.

Very truly yours, DAVID STARR JORDAN.
President Stanford University.

PLAN FOR A BIOLOGICAL SURVEY OF SOUTH AND CENTRAL AMERICA

By C. Hart Merriam

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WASHINGTON, D. C., *June* 13, 1902.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: I have the honor to submit herewith a plan, and to ask a grant for carrying on, a Biological Survey of Central and South America, with special reference to the terrestrial vertebrate fauna and the woody plants (particularly trees and shrubs), these types having proved the most useful in the study of geographic distribution.

Respectfully submitted. C. HART MERRIAM.

The Field.—While sporadic collections have been made in many parts of Central and South America, no consecutive systematic studies of the faunal areas have been attempted. It is probable that a biological survey of the kind here outlined would result not only in the discovery of a large number of genera and several thousand species new to science, but also in the accurate outlining of the life areas of the region, and in the acquisition of material which would admit of comprehensive studies as to the origin and relationships of the South American faunas.

Plan.—The plan contemplates the collection of material and the elaboration of results. This means the employment of a corps of competent trained field naturalists and the conduct for a number of years of field expeditions in unknown and little known regions. It is expected that the principal assistants would give part of their time to the personal direction of work in the field and part to the elaboration of results. The specimens collected, together with full field notes relating to the same, would be sent to the central office for study.

Since most of the type specimens of the known Central and South American species are in foreign museums, it would be necessary for those in charge of the work to visit those museums in order to ascertain just what these species are. An effort would be made to duplicate these types in order that we may have in this country the necessary units for comparison.

Methods.—The methods recommended are those of the Biological Survey of North America, of which your petitioner was the organizer and of which he has had continuous charge for the past 14 years. These methods, briefly stated, consist in the study of faunas and the collection of specimens in the field, followed by the elaboration of results. The field expeditions, which have now covered most of North America from southern Mexico to Hudson Bay, Great Slave Lake, and various parts of Alaska, are in charge of trained field naturalists who note the changes in passing from one faunal belt or area to another, establish points along their boundaries, and make collections of the characteristic species. The parties are small and compact, usually consisting of three men—the assistant in charge, a competent field collector, and a man who acts as cook and packer (or boatman). They travel by wagon, pack outfit, or boat, according to the requirements of the country. Experience has shown that a given number of men accomplish vastly more work when divided into small parties than when united in large parties.

Date of beginning.—As the preliminary arrangements would take some time, it is recommended that a definite answer be given as early as practicable, and, if favorable, that the initial appropriation be made available in September, 1903.

Cost.—As it is impossible to obtain at the outset a sufficient number of competent trained field naturalists, it is recommended that the work be begun with a rather small grant, and that thereafter the annual appropriation be increased in accordance with the following estimates:

Estimate for fall of 1903	$3,000

ESTIMATE FOR FIRST YEAR.

Salaries of two chief assistants, at $3,000	$6,000
Salaries of two field collectors, at $1,500	3,000
Salaries of two field collectors, at $1,200	2,400
Field expenses, including transportation, subsistence, collecting outfit, and materials	5,000
Tansportation charges on specimens to the United States	500
Salary of stenographer and clerk	1,000
Cases for specimens	500
Total	$18,400

ESTIMATE FOR SECOND YEAR.

Same as for first year with the following additions:	$18,400
One chief botanist (or dendrologist)	3,000
One assistant botanist	1,500
Traveling and field expenses of botanists	2,000
1 field naturalist, at $2,000	2,000
1 preparator at central office	1,200
Expenses at central office and other miscellaneous expenses	300
Visits to foreign museums to examine type specimens from South and Central America	500
1 administrative assistant at central office	2,000
Total for second year	$30,900

It is recommended that after the second year an annual increase of ten per cent on this amount be provided for at least ten years.

No estimate for illustrations and reports is here made, as it is supposed that these will come under the general provision for publications.

General remarks.—With the possible exception of Africa, less is known of the fauna of South America than of any other part of the world. In order to secure a broad and properly balanced view of the life of the globe, it is necessary to know approximately the number of generic and specific types of each region. Until this is known, the relative values of the various faunal provinces can not be properly determined.

Recent investigations of fossil faunas in South America have developed numerous and weighty facts, previously unsuspected, as to the extraordinary composition and unlooked-for relationship of the animals inhabiting this region in the past. But so little is known of the existing fauna that the application and significance of the fossil record with reference to existing life is meager and unsatisfactory.

By far the greater part of the zoological material heretofore collected in Central and South America has found its way to foreign museums, where it is widely scattered. Publications relating to it are mainly in foreign languages, and the descriptions of species are, as a rule, so unsatisfactory that reference to the specimens themselves is necessary for positive identification. This, in the case of American workers, necessitates a trip to foreign museums. There would seem to be every reason why America should take the lead in this work; and it should be done in a broad and thorough way, so that the results would be final and of permanent value.

ICHTHYOLOGY OF THE PACIFIC OCEAN

BY DAVID STARR JORDAN

Dr. Jordan has submitted a plan for completing the study of the Ichthyology of the Pacific Ocean that includes a series of expeditions extending over a period of three years. Dr. Jordan's letter is as follows:

STANFORD UNIVERSITY, CAL., *April* 9, 1902.

The writer, with his associates, Prof. C. H. Gilbert, Prof. O. P. Jenkins, and Messrs. Snyder and Starks, instructors in Stanford University, has long had in contemplation an Ichthyology of the Pacific Ocean. To this end, they have thoroughly explored the Pacific coast from Bering Sea to the Galapagos, having already described all the known species in this region, figuring many of them. With the aid of the *Albatross*, many or most of the fishes of Bering Sea have been made known. In company with Mr. Snyder, the writer spent the summer of 1900 in Japan, securing about 800 species, 250 of them new. Last summer, with Dr. Evermann and Dr. Jenkins, was spent in the collection of the shore fishes of Honolulu, in the interest of the United States Fish Commission, obtaining about 350 species. At present the *Albatross*, in charge of Professor Gilbert, is at work collecting the deep sea fishes and invertebrates of Hawaii. This summer the writer proposes to spend in similar work in Samoa. Besides these personal excursions and the work of Garman, Bean, and others, the writer has correspondents in Japan, China, Australia, the Philippines, and elsewhere, from whom important collections are frequently received.

To complete this work properly, competent collectors should visit Peru, Chile, Patagonia, Australia, China, the Okhotsk Sea, and several of the islands of the East Indies and Polynesia. For this purpose suitable men can be found who will do the work for their actual expenses. Some of these are instructors at Stanford University; others are advanced students. So far as the American shores and islands are concerned, we may count on the co-operation of the United States Fish Commission; and the funds of the Hopkins Laboratory of Stanford University have maintained expeditions to Japan, the Galapagos, Mazatlan, and Panama. The University of Tokyo will co-operate in Japan.

But these funds are not sufficient to finish the work—at least not under many years.

To complete the survey, twelve to fifteen expeditions would be necessary, each costing from $1,000 to $1,200, if two men are sent together, and in remote regions this should always be done. With the fishes large collections of invertebrates and of other vertebrates could also be secured without much extra cost.

To take care of the collections one curator should be paid, and an adequate supply of bottles and alcohol should be at hand. All this, with the expeditions, could be provided for with a sum of $7,000 yearly for three years, besides the pay of artists and typewriters. With a smaller sum the same results could be reached in longer time.

The work of study of the collections could be accomplished without expense by professors and advanced students. Artists and typewriters, however, must receive salaries, if employed. All new species should be figured, and it would be most desirable if in a final report *all* could be figured. The figures already made by the United States National Museum could, of course, be used for this purpose.

The present writer and his associates will devote their available time to this work, in any event, but unless especially accelerated it must outlast any one man's lifetime.

I commend this to the attention of the Trustees of the Carnegie fund as a means of aiding our knowledge of the science of Ichthyology, our knowledge of the geographical distribution of organisms, and, through the collection at the same time of marine invertebrates, of extending the range of that branch of zoology.

In case a grant should be made to this end, Stanford University will house the collections until studied, after which it will send the first series to the United States National Museum and distribute the rest to the museums of the world. The books necessary in this work are already in the possession of Stanford University. The means for publication of the work as a whole are not provided for.

In case this work seems a suitable one for aid by the Carnegie fund, I commend it to the attention of your honorable Committee. I am,

Very truly yours,

DAVID S. JORDAN,
President Stanford University.

EXPLORATION AND STUDY OF THE TROPICAL PACIFIC OCEAN

By Alexander Agassiz

Museum of Comparative Zoology,
Cambridge, Mass., *September*, 24, 1902.

My Dear Dr. Billings:

In accordance with my promise, I send you a short sketch of what might be accomplished by an expedition which would spend about three years in the tropical Pacific.

The points of interest are the hydrographic study of that area of the Pacific east of a line running from San Francisco to the Paumotus and from the Paumotus to the central part of Peru. With the exception of some work which I did in Central America off Panama and the Galapagos in 1891, from the Gulf of California to the northern part of Ecuador, nothing is known of the depth and character of the bottom of that great area. In connection with this work dredging should be carried on west of the area which I occupied into the deepest water of the eastern Pacific basin. The extension of the surface fauna in depth from the shore into deep water should also be investigated. The Central American coast north and south of the equator is admirably fitted for such study on account of its steep slope and the short distance to deep water from the coast.

Next, some time should be spent in dredging from the central part of each group of oceanic islands into deep water until one reached the abysmal fauna of the bottom of the Pacific to get thus some idea of the contrast there may be or the affinities, as may be the case, between these insular oceanic faunæ and the Pacific deep water fauna. The groups to be examined are Massason, the Society, the Paumotus, the Marquesas, the Cook, Samoa, Fiji, the Ellice group, Marshall and Carolines.

With this should be carried on deep sea tow net work and the study of the pelagic fauna associated with each group of islands. A study of the pelagic fauna should be made along the line of the equatorial and other great currents. The geology, botany, anthropology, and ethnology of these various archipelagoes should be studied. The time is coming, if it has not already come, when the natives of these various groups will have adopted the ways of modern civilization. It will then be impossible to learn anything of their former

language, habits, mode of life, customs, religion, and traditions unless they are studied within a reasonable time and before the flora has been modified by contact with civilization.

To carry out these investigations it will be necessary to have a twin screw steamer built for the purpose and properly equipped. The United States has no vessel adapted to that purpose. The *Albatross* is a single screw steamer and is altogether too small as a coal carrier to work with economy. Such a vessel should be 200 feet on the water line and have a beam of 32 feet; equipped it would probably cost $175,000. It would cost $75,000 a year to run the vessel. Three naturalists would be needed for the deep sea work. Two photographers and one artist would find ample occupation during the expedition. A couple of geologists, the same number of botanists and of ethnologists could during three years spent on these groups obtain information which probably could not be duplicated. If all this work could be under the supervision of one person it would be of great value. Previous expeditions have limited their work to a great extent to certain groups of islands, and in consequence what we know of the various groups is the result of the work of a number of individuals and not the résumé by a single person who has seen the whole. The scientific members of the expedition could be left at different groups, depending upon local transportation, and they could be picked up on completion of their work by the steamer and transferred to the next group. A period of three years' work would add immensely to our knowledge of the hydrography, the fauna of the Pacific, and of the great Oceanic Island groups.

The expenses of the scientific assistants would be from $20,000 to $25,000 a year, and the cost of publishing the results of this expedition, while depending upon the amount of material collected, would certainly, if done in a creditable way, involve an expenditure of $200,000 to $250,000 spread over a period of ten to fifteen years, or say an annual expenditure of $20,000 at the outside.

The above is a fair statement of work in an interesting field which I do not think any government is likely to take up. A German deep-sea expedition has lately returned from the Indian Ocean; the English after their expedition with the *Challenger* are not likely to enter the field again; the Prince of Monaco and the French have limited their work to the Mediterranean and the western coast of France, and no other nation is likely to enter into competition with us. The

United States will probably limit its marine work to what the Fish Commission can do with the *Albatross,* and, with the exception of some occasional outside work, the Fish Commission is hardly in condition to do more than attend to the problems that they have in hand.

On the termination of the expedition the steamer could do some physical and chemical work into deep water, either off the Atlantic or Pacific coast, in connection with a well equipped chemical laboratory on shore and finish accurately what can only be done roughly on board ship.

Yours truly, A. AGASSIZ.

BIOLOGICAL EXPERIMENT STATION FOR STUDYING EVOLUTION

BY ROSWELL H. JOHNSON

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NEED OF EXPERIMENTAL STUDY OF EVOLUTION.

Our knowledge of the processes of evolution has been greatly retarded by lack of experimental investigation. Nearly all of the post-Darwinian writing has been either largely deductive or else upon the variation of individuals at a particular time and place, *i. e.,* static. Evolution, above all other things, requires dynamic studies. Much dispute centers around such observation because of various causes which may have been operative. Experiment alone admits exclusion of possible interfering elements. The various points in dispute are nearly all capable of decisive experimental testing; for instance, selective life values of different variations, inheritance of acquired characteristics, environmental alterability of the germ-plasm; modification preventing death under adverse condi-

tions, etc. So clearly has this need been seen that hardly an evolutionary writer of prominence has not appealed for such work. I remember that Professor Davenport in his course on evolution at Harvard made a strong plea for experimental work.

It is gratifying to see some start now being made in this direction. Professor Ewart, of the University of Edinburgh, in Pennycuik farm is carrying on experiments in heredity with birds and mammals. The results on telegony are the most valuable ever produced, and have practically settled, for most biologists, that former moot question. De Vries, in Holland, is experimenting upon large series of plants in extensive gardens with reference to heredity and variation. His classic work, the *Mutations Theori* is founded upon the results of these experiments.

Why Such Study Has Hardly Begun.

There is not lacking ability, interest, or desire to do work of this kind, but it is the lack of facilities which prevents effort. Such experiments are beyond the means of the unassisted worker for the following reasons:

(*a*) *Expense.* A barn, greenhouse, or a large and adequately protected garden is required. The collection of the material desired would sometimes require the expense of traveling.

(*b*) *Time.* Every day-year-around attention is impossible for most college teachers, who are generally absent for lectures, meetings and vacations, from time to time, and who can not afford to employ a reliable and skilled assistant to carry on the routine work in their absence.

(*c*) *Permanence.* Such experiments often need to be continued through many years. This is very difficult in a university where such work would probably be done by the energy of but one man who might at any time be called to another position.

(*d*) *The agricultural experiment stations are limited to economic problems.*

Advantages of an Experiment Station.

It is, therefore, easy to see why such work has not been carried on in the past, except to a very small extent, and why we may expect to see valuable results from such an institution. A further consideration, however, is that effort in such vivarium must be vastly more productive than equal effort by individual investigators, because:

(*a*) Much of the labor involved is of a routine nature, which can be carried on by a moderately skilled gardener or attendant, and thus the results may frequently be of greater value, because experiments involving numbers may be prosecuted on a larger scale and more problems undertaken.

(*b*) *Superiority of Equipment.* The equipment would be of the best, thus insuring more and better results. Accidents resulting from improvised apparatus have spoiled many experiments, as witness the work upon breeding caterpillars.

The problems to be undertaken are numerous; the most important would seem to be those mentioned in the introductory paragraph.

A few specific cases may be briefly cited:

Problems to be Solved.

1. Environment alterability of the germ-plasms.

(*a*) Peas are said, by Bailey, to be particularly susceptible to the wetness of the soil in which they grow. Grow sweet peas which self fertilize in a heavy muck and keep well watered. Grow others in a loam never watered if outdoors. Grow the progeny in the respective environments for many generations; observe, first, what divergence takes place in one generation; and, second, to what degree, if any, the divergence is cumulative in successive years. *Each* year grow some plants from seeds of each set in an intermediate environment. Do they revert completely to a common type each year, or are they found to retain, each year, more of their divergence?

(*b*) Bring into cultivation some wild plants of a species which has given rise to a cultivated variety. Carefully avoid selection. Observe whether successive generations, nevertheless, diverge from the wild species.

2. Inheritance of somatogenetic characteristics.

(*a*) Some species, as the painted turtle, which both swims and walks and has short generations, may be experimented upon in two series. In one case they are confined to water; in the other, kept from it. Observe as above.

(*b*) Grow Chologaster, from which the blind fishes are descended, in the dark. Observe as above.

3. Isolation.

(*a*) Isolate two groups, of ten each, of a species which differ abnormally in opposite directions from the normal conditions, *e. g.*, ten turtles lacking one pair of scutes and ten turtles with a supernu-

merary pair. In successive generations would they regress to the mean, according to Galton, or diverge, according to Gulick?

(*b*) Experiment similarly with two groups, not far from the mean, of some very variable species, as with fish with anal fin rays having a normal distribution from 8-14 with mode at 11, using the one group with 10, the other with 12. Observe as above.

4. Natural selection.

The life-death value of variations. Subject large numbers of *Palaemonetes vulgaris* (a marine species) to fresh water. Compare the survivors with the dead, in respect to number of rostral spines, because a fresh water species of the genus differs in having fewer spines, as do the races of this species from brackish water of very low salinity.

5. Fecundal selection.

(*a*) Grow a large series of several varieties of wax podded beans, which self fertilize. Record fecundity. Is it correlated with any other characteristics? In the first series grow seed from the five most fecund plants each year; in the second, grow seeds from five plants of mean fecundity. Do the series diverge in other respects than fecundity?

(*b*) Do the number of visits of insects vary throughout the flowering period of a species? If so, does the fecundity of the successive flowers correspondingly vary? Sow from the most fecund flowers and from those of average fecundity. Does the time of flowering diverge in the two series? Is any correlated character affected?

(*c*) In a racemose species, do the lowest flowers differ from the highest in fecundity? If so, grow seed from each. After several generations of such selection, do the plants differ?

(*d*) Same with the inside and outside florets of a sun flower.

6. Organic evolution.

A tree frog which has the power of very slowly acquiring the color of its environment may be grown in a green environment.

Those which become the greenest would be artificially selected, in the successive generations.

In another lot, grown under ordinary conditions, select the greenest in the successive generations.

Is the green color acquired more quickly in the first case, when the selection is along the line of individual accommodation, than in the second case?

7. Sport prepotency.

(*a*) Cross an albino of a species of a genus which has white species with the normal form; observe potency of the sport.

(*b*) Isolate an albino with nine normal individuals, four of same sex as albino and five of the other.

After many generations is the white swamped or does it prevail?

8. Do likes tend to breed together, thus establishing segregation and divergence?

This could be observed in Leghorn fowls with rose comb and with single comb; also with Buff fowls which have many white tail and wing feathers and those which are almost free from them.

9. Correlation.

Select solely for one character. Are many correlated characters also intensified?

10. Determinate variation.

Grow one species, like *Oenothera lamarckiana,* known to have determined variation, permitting all seeds to grow or selecting at random for successive generations. Do the "*petites especes*" become more prominent?

Caution in all experiments. Record all deaths, in order that any influence of natural selection in the results may be recognized.

Equipment and Cost.

The Vivarium would, in general aspects, resemble an agricultural experiment station, and could be creditably and efficiently run within the limits of the size and cost of the several agricultural stations of the country. I shall outline what I have in mind, although either retrenchment or enlargement would be possible within certain limits, affecting only the amount and variety of results.

(*a*) *Greenhouse.* This should be divided to give three temperatures, a large middle house with two smaller portions partitioned off for cooler and hotter temperatures. All three portions should be large enough to give space for aquaria, insect cages, and the like, as well as for plants.

(*b*) *Barn.* This should be long, narrow, and one story high. On either side of the aisle would be wired cages, each communicating with wired runways outdoors, designed for cats, rabbits, guinea-pigs, or fowls.

(*c*) *Laboratory building.* This should have two stories and a basement, and should adjoin the barn and greenhouse. This would contain a general work and store room and several laboratory rooms.

The construction of all buildings should have permanence its chief end; hence they should be fireproof throughout. The initial equipment would consist of gardening and greenhouse tools, microscope, microtome, reagents, photographic apparatus, measuring apparatus, computing tables, instruments, glassware, trays, aquaria, wire cages, etc. Other equipment that would be needed as the experiments demanded would be seeds, plants, animals, feed, chemicals, etc.

Location and Grounds.

The laboratory should be situated on a protected marine shore, near a fresh water pond. The grounds should contain a garden of two acres and cement walled ponds. The location should be far enough south to have a long growing season and to economize fuel. Cheap lands and good collecting grounds necessitate a country location, but it should be situated within easy range of a large city, preferably Baltimore, where purchases can be quickly made and the necessary books loaned or consulted. I consider that $10,000 would cover the erection and original equipment.

Maintenance.

The regular staff would consist of a director and an assistant director, who would be men of scientific attainments in this line, and also of two attendants, one who has skill as a gardener and the other as a care taker of animals.

The plans provide two extra laboratory rooms on the second floor for additional workers. $1,800, $1,500, $800, and $700 might, I think, suffice as salaries. Another $1,500 a year would be necessary for materials needed and for minor expenses, bringing the annual maintenance expense to $6,300. Doubtless the results of the work could be published in existing journals without expense to the Vivarium.

Time.

One year should suffice to complete the building and begin operation.

STANFORD UNIVERSITY, CAL., *August* 28, 1902.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: I have received a copy of an outline of a plan for a proposed Vivarium for Experimental Evolution, suggested by Mr. Roswell H. Johnson, of the University of Wisconsin.

Permit me to say that, in my judgment, the establishment of such an institution, if placed in charge of some man of high skill in handling this class of experiments, would be an extremely desirable piece of work in the line of advanced research, and I trust that the Committee of the Carnegie Institution in charge of this matter will give it careful consideration.

Very truly yours,

DAVID S. JORDAN.

BIOLOGICAL EXPERIMENT STATION FOR STUDYING EVOLUTION

BY CHARLES B. DAVENPORT

ZOOLOGICAL LABORATORY,
UNIVERSITY OF CHICAGO, *May* 5, 1902.

To the Trustees of the Carnegie Institution.

GENTLEMEN: I beg leave to present for your consideration the following proposal for the establishment and maintenance by the Institution of a Biological Experiment Station for the study of evolution.

1. *Aims*. The aims of this establishment would be the analytic and experimental study of the causes of specific differentiation—of race change.

2. *Methods*. The methods of attacking the problem must be developed as a result of experience. At present the following seem most important:

(*a*) Cross breeding of animals and plants to find the laws of commingling of qualities. The study of the laws and limits of inheritance.

(*b*) The experimental production of variation, both by internal operations, such as hybridization, or by change of external conditions.

(*c*) Study of normal variation, especially as associated with changes in habitat and with geographical distribution—the effects of isolation.

(*d*) Experimental study of the effects of selection and of the origin of adaptation in organisms. Does morphological adaptation precede or follow change of habitat?

3. *Importance.* The scientific need of such a station has long been recognized. Bacon recognized its importance and dreamed of it in the new world. In later times Whitman and Osborn in this country, Romanes in England, and De Varigny in France have pointed out the scientific necessity for such an institution. Our agricultural experiment stations fall far short of meeting the scientific requirements, because their work has to be of an immediate practical turn. The Carnegie fund offers the opportunity for which the world has so long been waiting.

4. *Requirements.* To carry out the program proposed the following will be required:

(*a*) Plot of ground in the country, near the sea, presenting a great variety of conditions, not too distant from a scientific center with its libraries.

(*b*) Building provided with a greenhouse, constant temperature, and dark underground rooms, aquaria, insectaries, mammal rooms, administrative offices, and laboratories.

(*c*) Scientific staff, consisting of a director and two assistants; also two or more laborers.

5. *Estimate of Expenses:*

Initial.

Cost of ground, say 20 acres, about	$10,000
Cost of laboratory buildings and furnishings	25,000
Cost of putting grounds in order, seeding, fencing etc.	500
First cost of stock	500
Total initial expense	$36,000

Annual.

Salary of Director	$4,000
Salary of two assistants	2,000
Wages of laborers	1,200
Feed	300
Fuel	250
Books, stationery, etc.	750
Repairs, additions, and laboratory supplies	500
Total annual expense	$9,000

6. *Specific Proposals.* That this station be established at Cold Spring Harbor, which has the advantages of abundant fresh water, proximity to the sea, good sanitary conditions, great variety of environments, proximity to New York, to the Biological Laboratory, and to the New York Fish Hatchery.

Respectfully submitted,

CHARLES B. DAVENPORT.

ESTABLISHMENT OF A BIOLOGICAL FARM

By Dr. C. O. Whitman

The following is quoted from the report of Dr. C. O. Whitman, on a biological farm in connection with the Marine Biological Laboratory at Woods Hole, Massachusetts.

The fundamental problems of heredity, variation, and evolution can not be wholly considered in the laboratory. They concern vital processes known only in living organisms—processes which are slow and cumulative in effects, expressing themselves in development, growth, life histories, species, habits, instincts, intelligence. These problems require, therefore, to be taken to the field, where the forms selected for study can be kept under natural conditions, and where the work can be continued from year to year without interruption. Such a field, combining land and water, and stocked with animals and plants, and provided with a staff of naturalists, would have the essentials of a biological farm, now justly considered to be one of the great desiderata of biology.

This great need has been felt ever since Darwin's time, and has been strongly urged by such evolutionists as Romanes, Varigny, Galpon, Weismann, and Mendola. Thus far the project has not been realized, except on a small scale, through individual effort.

The functions to be fulfilled by a biological farm are the deep and broad needs of pure research on living organisms. The problems of heredity and variability are fundamental and naturally form the center of interest. Variability is the source of new species and the fountain of all progressive development in the organic world. In heredity lies the power of propagation and continuity of species. These are inexhaustible subjects, from the investigation of which must flow rich accessions to knowledge, which will redound to the

advancement of human welfare. The functions of a biological farm are not all summed up in experimentation. That old and true method of natural history observation must ever have a large share in the study of living things.

The biology of today is not too much laboratory, but too little of living things.

Outlay and maintenance.—The original outlay for land, stock, buildings, equipment, inclosures of land and water for isolation purposes, would vary according to the forms selected for study. From $50,000 to $100,000 would suffice for this. The maintenance of the first section, including salaries, accessions to stock, library, etc., may be estimated at $10,000 a year. The cost of additional sections would be about $5,000 each.

PROPOSED ANTARCTIC EXPEDITION

CARNEGIE MUSEUM, PITTSBURG, PA.,
April 21, 1902.

To the Executive Committee of the Carnegie Institution.

GENTLEMEN: Inasmuch as the same generous donor who has called into being the institution which you represent is the founder for like ends of the institution which we have the honor of representing, we are gratified by the receipt of your letter asking for suggestions as to any special line of scientific research to which in our judgment a portion of the funds of the Carnegie Institution might be advantageously devoted.

In reply to your communication we beg to submit the following as worthy of your earnest consideration:

The Carnegie Museum of Pittsburg, Pa., is desirous of despatching an expedition, under the leadership of Mr. J. B. Hatcher, to the Antarctic regions, and more particularly to Graham Land and the adjoining seas and islands.

The chief purposes of this expedition will be, *first,* to study the geology, paleontology, and biology of the lands and waters of this region, with special reference to their bearing upon the supposed former land connection between South America and Antarctica, and to discover, if possible, the nature, duration, and time of such connection; *second,* to make as complete collections as possible of the marine and terrestrial vertebrate and invertebrate faunas

and of the floras of this region; *third,* to conduct two lines of soundings, one from Cape Horn to Graham Land by way of the Dirk Garritz archipelago, and a second from South Georgia by way of the South Orkneys. It is proposed also to devote considerable attention to the geology and biology of the South Orkneys, South Georgia, and the Fuegian archipelago and their waters.

For the proper accomplishment of the work of this expedition it is proposed to place the geological and paleontological work under the direct charge of the leader of the expedition, and to engage the services of competent invertebrate and vertebrate zoologists to take direct charge of the work in those departments, supplying each head of these three departments with experienced assistants. The salaries of the scientific staff of the expedition will be met by the funds of this museum.

For transporting the expedition to and from the Antarctic region and for facilitating its labors while in that region, it is proposed to purchase, or charter for a period of two years, commencing August 1, 1903, an efficient and serviceable steam whaler or other vessel, and to fit her for deep sea soundings and for dredging to moderate depths, say from 500 to 800 fathoms. She should also be provided with suitable laboratory facilities for the zoologists of the expedition.

In order that this work may be carried out successfully and with efficient funds at its disposal, we, the undersigned officials of this museum, respectfully request that on August 1, 1903, a fund of $10,000 be set aside for securing by charter or purchase a suitable vessel and providing the necessary equipment of the same.

And, further, that on the first day of August, 1904, and on the same day of the same month, 1905, two further sums of $25,000 each be set aside for the expenses of the expedition so far as they may be included in chartering, purchasing, equipping, and maintaining the vessel, paying the crew, and in general conducting the expedition; all amounts to be accounted for and any balance not expended to be returned to the treasury of the Carnegie Institution.

C. C. MELLOR,
Chairman of Committee on Museum of the Board of Trustees of Carnegie Institute.
W. J. HOLLAND,
Director of the Carnegie Museum.
J. B. HATCHER,
Curator of Vertebrate Paleontology.

PROPOSED INVESTIGATION OF SUBTERRANEAN TEMPERATURES AND GRADIENTS

BY G. K. GILBERT

WASHINGTON, D. C., *November 19, 1902.*

To the Trustees of the Carnegie Institution.

GENTLEMEN : I beg to submit for the consideration of the Institution a proposed investigation of subterranean temperatures and gradients by means of a *deep boring* in plutonic rock.

Information as to temperature gradients in the earth's crust depends, up to the present time, on observations made in wells bored for economic purposes and in mines. Wells bored for economic purposes penetrate the sedimentary rocks, because such rocks contain the potable waters, brines, petroleums, gases, and salt deposits which are sought. For various reasons these rocks should not be expected to give the normal temperature gradient of the earth's crust. First, they are heterogeneous, and as different layers differ in heat diffusivity, the gradient is modified thereby ; second, the temperatures of the sedimentary rocks are, as a rule, modified by the circulation of water ; since heat is thus distributed by convection, the conditions are not favorable for the determination of the facts of conduction ; third, the sedimentary rocks are not representative of the crust as a whole ; therefore inferences from their temperature gradients and other heat phenomena can not be applied to the igneous or plutonic rocks which make up the body of the crust.

Temperature observations in mines are also subject to exceptional conditions, since the ore deposits followed by deep mines are often associated with comparatively recent diastrophic or volcanic disturbances, and are usually routes of water circulation.

The student of the problems of the inner earth needs information as to the thermal conditions of a homogeneous rock mass representative of the crust as a whole. To this end it is desirable that a boring be made, as deep as possible, in a plutonic rock. A mass should be selected which is of great age and which has not for many geologic periods been subjected to diastrophic changes. A mass having large superficial extent and presenting uniform characters throughout its area could presumably be penetrated to a great depth without encountering important changes in composition.

The method of boring adopted should be one which yields a core, so that physical investigations can be made of the rock penetrated

at various depths. Rock specimens from the core should be examined in the laboratory to determine their heat diffusivity, and this should be investigated (1) in relation to varying pressure, and (2) in relation to varying temperature.

The results to be hoped from such an investigation are:

1. A determination of the general temperature gradient of the upper part of the crust—a determination having higher authority than any previously made.

2. A determination of the downward variation of temperature gradient.

3. A redetermination of the rate at which heat reaches the surface through conduction in the crust.

4. A determination of any variation in the rate of heat movement in relation to depth. The character of such determined variation, or the determination of the absence of variation, bears on theories as to the source of the heat conducted toward the surface.

5. The detection of variations of gradient occasioned by Pleistocene oscillations of climate, and their possible interpretation in terms of time.

The value of such a research will depend largely upon the depth penetrated by the boring. I am informed by Mr. F. H. Newell, of the United States Geological Survey, that core drilling has heretofore been carried only to moderate depth, so that for the purposes of such an investigation a special plant, of an elaborate character, will be required. This plant should provide for the commencement of the boring with a large diameter and the progressive reduction of diameter as greater depths are reached. The plant should be carefully planned in advance, and such planning requires expert knowledge and skill. Mr. Newell informs me that the necessary knowledge and skill can be commanded, and mentions in that connection Professor Slichter, of Madison, Wisconsin.

I am not prepared to make recommendation as to persons by whom the proposed investigation should be conducted; but if the Institution shall establish a laboratory of geophysics the general direction of the investigation might advantageously be entrusted to the officers of that laboratory.

I estimate that the amount of money needed for the work will be $50,000, that the time probably covered would be three years, and that the need for the first year would be $10,000.

Very respectfully,

G. K. GILBERT.

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CARNEGIE INSTITUTION

OF

WASHINGTON

YEAR BOOK

No. 2

1903

PUBLISHED BY THE INSTITUTION
WASHINGTON, U. S. A.
JANUARY, 1904

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DANIEL C. GILMAN

Executive Committee

DANIEL C. GILMAN, *Chairman*
CHARLES D. WALCOTT, *Secretary*
JOHN S. BILLINGS
JOHN HAY
S. WEIR MITCHELL
ELIHU ROOT
CARROLL D. WRIGHT

Finance Committee

LYMAN J. GAGE
HENRY L. HIGGINSON
D. O. MILLS

Office of the Institution
Bond Building, Corner of New York Avenue and Fourteenth Street
Washington, D. C.

ARTICLES OF INCORPORATION

OF THE

CARNEGIE INSTITUTION OF WASHINGTON

We, the undersigned, persons of full age, and citizens of the United States, and a majority of whom are citizens of the District of Columbia, being desirous to establish and maintain, in the City of Washington, in the spirit of Washington, an institution for promoting original research in science, literature, and art, do hereby associate ourselves as a body corporate for said purpose, under An Act to establish a code of Law for the District of Columbia approved March third, nineteen hundred and one, sections 599 to 604 inclusive; and we do hereby certify in pursuance of said act as follows:

First. The name or title by which such institution shall be known in law is CARNEGIE INSTITUTION.

Second. The term for which said Institution is organized is perpetual.

Third. The particular business and objects of the Institution are the promotion of study and research, with power:

(*a*) To acquire, hold, and convey real estate and other property necessary for the purposes of the Institution as herein stated, and to establish general and special funds;

(*b*) To conduct, endow, and assist investigation in any department of science, literature, or art, and to this end to cooperate with governments, universities, colleges, technical schools, learned societies, and individuals;

(*c*) To appoint committees of experts to direct special lines of research;

(*d*) To publish and distribute documents;

(*e*) To conduct lectures;

(*f*) To hold meetings;

(*g*) To acquire and maintain a library;

(*h*) And, in general, to do and perform all things necessary to promote the objects of said Institution.

Fourth. That the affairs, funds, and property of the corporation shall be in general charge of a Board of Trustees, the number of whose members for the first year shall be twenty-seven (27), and shall not thereafter exceed thirty except by a three-fourths vote of said Board.

IN TESTIMONY WHEREOF we have hereto set our names and affixed our seals, at the City of Washington, in the District of Columbia, on the fourth day of January, 1902.

JOHN HAY [SEAL]
EDWARD D. WHITE [SEAL]
JOHN S. BILLINGS [SEAL]
DANIEL C. GILMAN [SEAL]
CHARLES D. WALCOTT [SEAL]
CARROLL D. WRIGHT [SEAL]

DISTRICT OF COLUMBIA : *ss*:

Be it remembered that on this 4th day of January, A. D. 1902, before the subscriber personally appeared the above named John Hay, Edward D. White, John S. Billings, Daniel C. Gilman, Charles D. Walcott, and Carroll D. Wright, to me personally known and known to me to be the persons whose names are subscribed to the foregoing instrument of writing, and severally and personally acknowledged the same to be their act and deed for the uses and purposes therein set forth.

Given under my hand and official seal the day and year above written.

WILLIAM MCNEIR,
Notary Public.

[SEAL]

BY-LAWS

OF THE

CARNEGIE INSTITUTION OF WASHINGTON

1. The officers of the Board of Trustees shall be a Chairman, a Vice Chairman, and a Secretary, all of whom shall be chosen biennially, by ballot.

2. The annual meeting of the Board shall be held in Washington, on the second Tuesday of December, beginning with the year 1903. Other meetings of the Board may be called by the Executive Committee on twenty days' notice to each member of the Board, and they shall be called in the same manner by the Chairman of the Board or by the Secretary, on the written request of seven members of the Board.

3. The Trustees, by ballot, shall appoint a President of Carnegie Institution, whose term of office shall be five years. He may or may not be a member of the Board of Trustees.

4. The Trustees, by ballot, shall designate seven Trustees as an Executive Committee. Their terms of office shall be three years, and the members shall be reëligible. The Committee shall determine by lot the term of office. Any person elected to fill a vacancy shall be chosen for the unexpired part of his predecessor's term.

5. The fiscal year of the Institution shall be from November 1 to October 31, inclusive.

6. There shall be a Finance Committee consisting of three members of the Board, to be elected by the Board, to hold office until their successors are elected. The duty of such Finance Committee shall be to consider and recommend to the Board of Trustees such measures as it may believe will promote the financial interests of the Institution.

7. The Executive Committee shall, when the Board is not in session and has not given specific directions, have charge of all arrangements for administration, research, and instruction; designate advisory committees for specific duties; determine all payments and salaries; keep a written record of all their transactions and expenditures, and submit to the Board of Trustees at the annual meeting a report

which shall be published: *Provided*, That no expenditure shall be authorized or made by them except in pursuance of a previous appropriation voted by the Board.

8. The Executive Committee shall make arrangements for the custody of the funds of the Institution; keep an accurate account of all receipts and disbursements, and submit annually to the Trustees a full statement of the finances of the Institution, and a detailed estimate for the expenditures of the succeeding year.

9. In the event of a vacancy in the office of President, or in his absence or inability to perform his duties, they shall devolve upon the Secretary of the Executive Committee, who shall be a member thereof.

10. At least one month before the annual meeting, the Chairman of the Board of Trustees shall appoint a skilled public accountant to audit the accounts of the Institution.

11. The terms of all officers shall continue until their successors are elected.

12. These By Laws may be amended at any meeting of the Board of Trustees by a majority vote of the entire membership of the Board, provided written notice of the proposed amendment has been mailed to each member of the Board thirty days prior to the meeting.

MINUTES OF THIRD MEETING OF BOARD OF TRUSTEES

[Abstract]

The meeting was held in Washington, at the New Willard Hotel, on Tuesday, December 8, 1903, at 10 A. M.

The Vice Chairman, Mr. Billings, occupied the chair.

The Secretary called the roll, and the following Trustees responded to their names:

J. G. CANNON, Speaker of the House of Representatives

ALEXANDER AGASSIZ, President of the National Academy of Sciences

JOHN S. BILLINGS	WILLIAM LINDSAY
WILLIAM N. FREW	WAYNE MACVEAGH
LYMAN J. GAGE	D. O. MILLS
DANIEL C. GILMAN	S. WEIR MITCHELL
HENRY L. HIGGINSON	WILLIAM W. MORROW
E. A. HITCHCOCK	ELIHU ROOT
CHARLES L. HUTCHINSON	CHARLES D. WALCOTT

CARROLL D. WRIGHT

Absent:

THE PRESIDENT OF THE UNITED STATES

WM. P. FRYE, President of the Senate

S. P. LANGLEY, Secretary of the Smithsonian Institution

JOHN HAY	JOHN C. SPOONER
SETH LOW	ANDREW D. WHITE

Letters were received from Messrs. Frye, Hay, Low, Spooner, and White, regretting their inability to be present at the meeting.

The minutes of the second meeting were read and approved.

A resolution inviting Mr. Carnegie to attend the meeting was unanimously adopted, and in accordance therewith an invitation was sent to Mr. Carnegie, and a few minutes later he entered and remained during the session.

The Chairman announced the death of Mr. Abram S. Hewitt and Mr. William E. Dodge. Mr. Mills presented the following minute

relative to the death of Mr. Hewitt, which was read and adopted by the Board:

By the death of Hon. Abram S. Hewitt, Chairman of this Board, the Trustees are bereft of a colleague whose ability, experience, and character gave him the foremost place among those who were invited by the founder to initiate the plans of the Carnegie Institution. For these responsibilities he had exceptional qualities. A liberal education fitted him to take broad views of the needs of the country and of the progress of learning; during a long career in business he had been brought into close relations with many of the leaders in science and its applications; his services in Congress had made him familiar with the scientific departments of the government; his relations to the Cooper Union acquainted him with the aspirations and needs of promising youth; he was honored not only in the city which he served as mayor, but throughout the country, for his integrity and wisdom; in the projects of this Institution he was profoundly interested; he foresaw what good would come to the country and to the world from the judicious use of its resources. He brought to its service sagacity, enthusiasm, judgment, and strength. For these personal and official characteristics his name will be held in grateful remembrance in the annals of this foundation.

Mr. Hutchinson presented the following minute relative to the death of Hon. Wm. E. Dodge, which was read and adopted by the Board:

The first vacancy which occurred in the Board of Trustees was filled by the choice of Hon. Wm. E. Dodge. He was gratified by the appointment and in full accord with the purposes of the foundation; but soon after his election serious illness impaired his activity and his death occurred without his taking any part in our proceedings.

Mr. Dodge, like Mr. Hewitt, was a man of wide acquaintance with public affairs, who had been connected as trustee with many scientific and educational institutions, and had acquired by observation, reading, and association an acquaintance with the progress of many branches of knowledge, so that the most valuable help was expected from him.

The President of the Institution, Mr. Gilman, gave a brief review of the report of the Executive Committee, which had been printed and distributed to the Trustees in advance of the meeting.

The Secretary in his report referred principally to the financial transactions, and to the business that would come before the Trustees as resulting from the death of Messrs. Hewitt and Dodge, and the resignation of Mr. E. D. White, and the expiration of the terms of office of the officers of the Board and the members of the Executive Committee. He submitted the following financial statement and statement of assets:

CARNEGIE INSTITUTION OF WASHINGTON.

Financial Statement for the Period November 1, 1902, to October 31, 1903, Inclusive.

Receipts.		Amount.
Balance on hand November 1, 1902		$219,822 22
Received since:		
From interest on endowment	$500,000 00	
" interest on deposits with U. S. Trust Co.	5,788 09	
" interest on deposits with American Security and Trust Co.	79 01	
" Index Medicus subscriptions	2,256 91	
" sales of Year Book	29 25	
" sales of furniture	69 00	
" postage returned	27 21	
" gas deposit returned	5 36	
		508,254 83
		$728,077 05

Disbursements.			Amount.
For grants in aid of research		$135,617 02	
For publications		959 28	
For administration:			
Trustees' expenses	$950 14		
Executive Committee's expenses	1,014 46		
Honorariums and expenses of Advisers and Secretary	21,226 71		
Salaries of officers	13,000 00		
Pay of employés	2,692 23		
Rent, fuel, and light	2,564 18		
Furniture	1,235 28		
Stationery and office supplies	302 89		
Printing	1,722 90		
Postage, express, and telegrams	408 93		
Telephone	62 55		
Miscellaneous	373 79		
		45,554 06	
For purchase of bonds for reserve fund		100,475 00	
			$282,605 36
Balance on hand October 31, 1903:			
On deposit with U. S. Trust Co.		$445,135 31	
On deposit with American Security and Trust Co.		336 38	
			445,471 69
			$728,077 05

Assets of the Carnegie Institution.

In the endowment:		
One hundred registered 5 per cent. bonds of the United States Steel Corporation		$10,000,000
In the reserve fund:		
Northern Pacific Railway Company Land Grant General Mortgage 4's of 1997	$50,000	
Atchison, Topeka, and Santa Fe Railroad Company General 4's of 1995	50,000	
		100,000
Office outfit at Washington (estimated)		1,500
		$10,101,500

On the submission of the report of the Executive Committee, the chairman made a general statement supplementary to that made by the President, in relation to the results of the work accomplished by the Institution thus far, and of the scope of its future work as indicated by the grants recommended.

A somewhat extended discussion followed on the subject of minor and larger grants, and a series of resolutions was passed making the following general appropriations:

Reserve fund	$100,000
Publication fund, to be continuously available	40,000
Administration	60,000
Grants for large projects	130,000
Grants for minor researches	200,000
Special grants	43,000

Mr. Carnegie then expressed his satisfaction with the progress thus far made, and with the plans as outlined for the future, and thanked the Trustees for the work they are doing for the Institution.

At 12.30 the Board took a recess until 2 P. M.

At the second session there were present:

Prof. ALEXANDER AGASSIZ, President of the National Academy of Sciences

JOHN S. BILLINGS	WILLIAM LINDSAY
WILLIAM N. FREW	D. O. MILLS
LYMAN J. GAGE	S. WEIR MITCHELL
DANIEL C. GILMAN	WILLIAM W. MORROW
HENRY L. HIGGINSON	ELIHU ROOT
E. A. HITCHCOCK	CHARLES D. WALCOTT
CHARLES L. HUTCHINSON	CARROLL D. WRIGHT

The Board took up the question of a seal for the Institution, and referred the matter to the Executive Committee with power.

Amendments to the By Laws were then considered. By Law No. 10 was amended by substituting in the second line the word "skilled" instead of "authorized."

The Board then proceeded to fill the vacancies caused by the death of Mr. Hewitt and Mr. Dodge, and the resignation of Mr. E. D. White. The election resulted in the choice of John L. Cadwalader, New York; Cleveland H. Dodge, New York; and Wm. Wirt Howe, New Orleans.

The election of members of the Executive Committee to fill the places of Messrs. Gilman, Mitchell, and Wright, whose terms expired, resulted in their reëlection to the class of 1906.

Mr. John Hay was elected to succeed Mr. Hewitt on the Executive Committee in the class of 1905.

The Board then elected its officers to fill the vacancies caused by the death of Mr. Hewitt and by the expiration of terms of office of Vice Chairman J. S. Billings and Secretary C. D. Walcott as follows:

Chairman, J. S. Billings.

Vice Chairman, Elihu Root.

Secretary, C. D. Walcott.

The report of the Finance Committee was then read and approved.

At 3.10 P. M. the Board adjourned.

CHAS. D. WALCOTT,
Secretary.

REPORT OF EXECUTIVE COMMITTEE ON THE WORK OF THE YEAR

INTRODUCTION.

Immediately after the Board of Trustees adjourned, November 25, 1902, the Executive Committee began to consider its various directions, and also such matters as had been recommended by the Committee and approved by the Board.

During the fiscal year the Executive Committee held nine meetings.

Its organization continued the same as last year—Mr. Gilman, Chairman, and Mr. Walcott, Secretary. Mr. Marcus Baker continued as Assistant Secretary, taking charge of editing, printing, accounting, and the general direction of all routine work in the office.

GRANTS MADE AND REPORTS THEREON.

At the last annual meeting the Trustees set apart $200,000 for grants for research during the fiscal year 1902–1903. The following is a list of grants made by the Executive Committee under such authority. Each one is accompanied by a brief statement of the results thus far obtained. When an investigation is completed, a final report will be submitted by the grantee. This may be printed either in abstract or in full in the Year Book.

ANTHROPOLOGY.

G. A. Dorsey, Field Columbian Museum, Chicago, Ill. Grant No. 43. *For ethnological investigation among the Pawnees.* $2,500.

Abstract of Report.—This scheme of investigation will require four or five years for its completion. It is a study of the religious ceremonies of the Pawnee Indians, with direct reference to the mythological origin of each ceremony, and to obtaining a clear and comprehensive understanding of the religious systems of the Pawnees.

The work of collecting and arranging the details of the region of the religion was begun early in the year, and has been pushed forward as rapidly as possible. The work of the first year was to obtain the mythology of the Skidi on the one hand, and the Chaui, Kitkahahki, and Pittahauirata bands of Pawnees on the other, and of the Wichita and Arikara. The second result sought for was to gain a comprehensive insight into all the ceremonies of the four bands of the Pawnees and of the Arikara. Of these two results as much has been achieved as could be hoped for, inasmuch as the work has progressed for only about nine months.

With the beginning of the first of the Skidi ceremonies early next spring, it will be possible to select certain of the more important ones for more detailed observations. Thereafter each ceremony will be studied independently and in detail, and the observations thus made, together with the ritual as sung, will be prepared for publication.

Wm. H. Holmes, Director Bureau of American Ethnology, Washington, D. C. Grant No. 44. *For obtaining evidence relative to the early history of man in America.* $2,000.

The phenomena to be considered are scattered and obscure. The geological formations of both continents, ranging from Eocene to Recent, abound in various records, but investigation has been in the main desultory and unscientific, and the isolated observations are to-day without adequate correlation.

Mr. Holmes proposed to begin his work with the compilation of all data respecting previous investigations, and then to begin field work which should extend to deposits in caves and caverns where men have lived, and should also include their ancient sites, such as kitchenmiddens, shell heaps, and earthworks.

Abstract of Report.—The field work in this investigation was done mainly by Mr. Gerard Fowke, archeologist, who began work in Indiana and carried his examinations into Illinois, Kentucky, Tennessee, and Alabama, exploiting many caves and making careful investigation of a few. Results were distinctly negative with reference to the principal question at issue, the entire season's work having developed no fact that will tend to establish a theory of the great antiquity of man in America. The season's work, however, was not a failure on this account, since the question is one that must be solved, if not by the discovery of positive evidence, by establishing the universality of negative evidence.

Late in the season explorations were begun on the Atlantic slope by Mr. F. B. McGuire, archeologist, in the caves of the upper Potomac in West Virginia. Mr. Holmes personally made a reconnaissance in Georgia and Alabama for the purpose of collecting definite information regarding the caves of the south.

With the aid of Mr. F. B. McGuire and Dr. J. W. Fewkes, a cave in Porto Rico was explored without expense to the Institution.

The present report can be regarded as only one of progress, since Dr. Fewkes and Mr. McGuire are still in the field.

George F. Kunz, New York city. Grant No. 52. *To investigate the precious stones and minerals used in ancient Babylonia in connection with the investigation of Mr. William Hayes Ward.* $500.

Abstract of Report.—This is an investigation in coöperation with that of Mr. William Hayes Ward. It was deferred until winter in order to secure the coöperation of Mr. Ward after his return from his investigations in Europe.

William Hayes Ward, New York city. Grant No. 50. *For study of oriental art recorded on seals, etc., from western Asia.* $1,500.

Dr. Ward has been for fifteen years devoting his spare time to oriental archeology, with special reference to the beginnings of art and mythology, as shown in recovered monuments and especially in the seal cylinders, which preserve a large part of the early art. He has handled thousands of seals and has paper impressions of thousands. The investigation covers a period from about 4000 B. C. to about 400 A. D. and will include a study of the mythological representations and various designs, emblems, and inscriptions contained in them.

Abstract of Report.—During last summer Dr. Ward visited various museums in the United States and in Europe, where he examined the great collections of Paris and Berlin. Every facility was granted by the authorities in charge, and he made notes and obtained casts of such cylinders and seals as were required for his investigations. He is now engaged in the preparation of manuscript and illustrations. It is estimated that it will require about two years to complete the study and prepare the results for publication.

ASTRONOMY.

Lewis Boss, Dudley Observatory, Albany, N. Y. Grant No. 7.
For astronomical observations and computations. $5,000.

Abstract of Report.—This work has for its ultimate object an investigation upon the motions of the brighter stars (all down to the seventh magnitude), and of all stars, of whatever magnitude, supposed to have motions as great as 10″ per century, and of many other stars which were specially well determined prior to 1850.

During the year Professor Boss's attention was given to—

(*a*) The compilation for each star of all observations for position that have been made upon it during the history of astronomy. Some stars are found in more than sixty catalogues.

(*b*) Investigation of the systematic errors with which each series of meridian observations seems to be affected, in order that the precision of the results may be notably increased. This involves in the first place the establishment of a standard of reference, which must include the positions of all those stars which have been most frequently and accurately observed.

The entire work is proceeding upon a logical plan carefully studied and formulated through the results of experience during past years, with a view to economy in the succession of individual investigations designed to contribute to the final result. In an extensive investigation of this kind there is always an element of danger. If the work is so planned that definite results cannot be realized until the completion of the whole work, there is liability to serious loss from the ordinary accidents of life which cannot be foreseen. Therefore this work has been so planned that useful results can be secured and promptly published at every successive stage of the work. Each step grows logically out of those which have preceded it. The computations are so planned that successive improvements in the fundamental basis can be introduced with the least possible duplication of work.

It is intended that the catalogue of more than 2,500 standard stars shall be offered for publication to the Carnegie Institution early in 1905, and if no unforeseen accidents occur this program should be entirely feasible.

During the present year the catalogue of 627 standard stars has been passing through the press and is now nearly ready for issue. Subsidiary investigations connected with this catalogue have been carried out under the grant of the Institution for this year.

Boss, Hale, and Campbell. Grant No. 70. *For investigating proposal for a southern and a solar observatory.* $5,000.

In the Year Book for 1902 a proposition for the establishment of a distinctly solar observatory was presented by Professor S. P. Langley. In the same report (page 89) the astronomical advisers called attention to the lack of observatories in the southern hemisphere, and in an appendix (pages 99 to 104) they treated the subject still more fully.

In order that the Board of Trustees might be enabled to arrive at appropriate conclusions, Professor Lewis Boss, chairman; Professor George E. Hale; and Professor W. W. Campbell were requested to investigate, as a Committee, the subject more fully and to consider the question of suitable sites for such observatories.

The result of the work of this committee is submitted in the Accompanying Papers of this Report, pages 1–70.

W. W. Campbell, Lick Observatory, Mt. Hamilton, Cal. Grant No. 53. *For pay of assistants to take part in researches at the Lick Observatory.* $4,000.

Abstract of Report.—Owing to the difficulty of obtaining satisfactory assistants from the east and providing living quarters for them on the mountain, it was not found possible to provide for an effective use of the grant for the employment of assistants and computers until late in the year.

Investigations were begun with the meridian circle work and in spectroscopy. With the construction of additional residence quarters on the mountain, Professor Campbell will soon employ the full number of assistants rendered possible by the grant.

Herman S. Davis, Gaithersburg, Md. Grant No. 11. *For a new reduction of Piazzi's star observations.* $500.

American and European astronomers have urged that a fresh reduction of these observations by known methods for obviating certain errors should be made. Professor Porro, of Turin, undertook a part of the reductions and Professor Davis the rest. Assistance from private persons and from observatories have contributed to the prosecution of this undertaking. The Carnegie Institution was asked to make a small contribution.

Abstract of Report.—The work accomplished under this grant has been in connection with work that was already begun. This makes

it difficult to define specifically the exact amount done under the grant from the Carnegie Institution. The period of nine months, during which the grant has been available, has marked the transition from the routine work of reducing the observed "apparent" positions of the stars to a common "mean" epoch to the next large step of deducing therefrom the instrumental errors and compiling the final catalogue. This rendered it necessary to spend this time in rounding out and perfecting all the divers portions of the computations which have been going on uninterruptedly for the past seven years. This has been finished, and also some preliminary work done for the next great and distinct stage of the work:

(*a*) To deduce the errors of the telescope for each night of observation.

(*b*) To correct all observations for these maladjustments.

(*c*) Finally, to combine the definite separate positions into means for each star included in the catalogue, which is the goal of the long labor.

George E. Hale, Yerkes Observatory, Williams Bay, Wis. Grant No. 13. *For measurements of stellar parallaxes, solar photographs, etc.* $4,000.

Abstract of Report.—Work was begun on the photographic investigation of stellar parallaxes early in May with a 40-inch telescope. Up to October, 114 plates, containing about 350 exposures, had been obtained. These included:

(*a*) Twenty experimental plates.

(*b*) Eighty-eight plates suitable for parallax determinations.

(*c*) Six plates of loose star clusters.

Considerable work was also done in the measurement of photographs of star clusters.

Another line of investigation was the photometric determination of stellar magnitudes. Considerable progress was made in this, fields being measured with the 6-inch reflectors and the 12 and 40-inch refractors. Measures were also made upon the Pleiades group of stars to determine the constant of the equalizing wedge photometer. Measurements were also made of comparison stars for faint variables.

Much progress was also made in the measurement and discussion of photographs of the sun, taken with the spectroheliograph at the Kenwood Observatory in the years 1892–1896, and in other minor investigations connected with the work in hand.

Simon Newcomb, Washington, D. C. Grant No. 17. *For determining the elements of the moon's motion and testing the law of gravity.* $3,000.

Much of the material for this investigation, consisting of computations of places of the moon from Hansen's tables and their comparison with observations, was preserved in the archives of the Nautical Almanac Office, waiting an opportunity for their working up. By permission of the Secretary of the Navy, Hon. William H. Moody, these papers were entrusted to the Carnegie Institution and by the Institution to Professor Newcomb.

Abstract of Report.—The importance of this work grows out of the fact that new tables of the moon are urgently required for the purposes of astronomy and of navigation. For a long period the problem of constructing and perfecting such tables has been delayed by an unexplained discordance between the observed motion of the moon and the motion which should result from the action of all known bodies upon it. The exact cause of this discordance cannot be recorded, because the observations from 1750 to 1850 have never been worked up and compared with the tables. The problem of determining the exact nature of the deviation of the moon from its predicted place is twofold. The observations since 1750 must be worked up, and in order to compute the comparison the action of the planets on the moon must be recomputed with a view to determining whether any correction to the past computations is necessary.

By aid of a grant from the Carnegie Institution an important term of long period, produced by the action of Venus, has been recomputed.

Professor Newcomb has taken up the work on the adopted plan of the occultations of stars by the moon, a work that he had begun in connection with the Nautical Almanac. This, in connection with the incorporation of other important observations, can probably be completed in two years more.

E. C. Pickering, Harvard University, Cambridge, Mass. Grant No. 20. *For study of the astronomical photographs in the collection of Harvard University.* $2,500.

Abstract of Report.—The grant made to Professor Pickering was applied to a great variety of uses. These included sums paid to 19 different assistants and computers, and for other assistance in connection with the Harvard Observatory.

Each of the numerous investigations is of importance in carrying forward the work going on in the observatory, but they do not appear to be upon sufficiently definite and specific problems, as given in his report, to permit of a distinct statement, in most cases, of the progress of the work under the Carnegie Institution grant.

Professor Pickering reports that in forming a corps of observers to study the photographs, time and money being limited, it was difficult to decide what subjects to select from this vast amount of material. A number of problems have accordingly been studied which serve to illustrate the various investigations which might be undertaken. Abridged results of a portion of these were promptly published in the Harvard Observatory Circulars Nos. 69 and 70. The principal researches carried on are as follows:

1. Eclipses of Jupiter's satellites.
2. Light curves of Algol variables.
3. Position and brightness of stars in clusters.
4. Observations have been made of the changes in light of 9 variable stars of long period, during several years before they were discovered.
5. Early observations of stars of the Algol type and other variables of short period.
6. Transit photometer.
7. Nova Geminorum.
8. Variations in brightness of Eros.
9. Proper motion of stars.
10. Missing asteroids.
11. Many images of interesting objects like new stars, variables, and asteroids doubtless appear on the photographs. An examination has accordingly been made of several of the plates to determine whether it would be advisable to examine a large number of them systematically for the discovery of such objects.

Wm. M. Reed, Princeton Observatory, Princeton, N. J. Grant No. 54. *For pay of two assistants to observe variable stars.* $1,000.

Abstract of Report.—Owing to the difficulty of obtaining an observer, work was not begun till March 1. During the seven months from March 1 to October 1, the 23-inch telescope of the Halsted Observatory, exclusively for photometric work, was used on every clear night from early in the evening until daylight. In all, 9,015 observations were made on about 50 different stars.

Three classes of stars were observed:

(*a*) Such variable stars as are too faint to be reached by any except the largest telescopes. In particular, selection was made of stars that have become too faint for the Harvard observers and those coöperating with them.

(*b*) Measurement of faint stars that are to be used as standards of magnitude. In this work they are connecting stars of the 13th magnitude with those of the 15th magnitude. The Lick and Yerkes observatories are connecting the 15th magnitude stars with the 16th magnitude, and the Harvard Observatory is connecting the 11th magnitude with the 13th magnitude.

(*c*) A special study of the newly discovered Algol variable, 4.1903 Draconis, has been made, and a preliminary article giving the results of these observations has been sent to the *Astronomical Journal.*

Mary W. Whitney, Vassar College, Poughkeepsie, N. Y. Grant No. 23. *For measurement of astronomical photographs, etc.*
$1,000.

Abstract of Report.—This work consists in the measurement and reduction of stellar photographs taken at the observatory at Helsingfors, Finland, by Professor Donner. The measurement of the eight plates is finished and the reduction is well along. A preliminary catalogue of the mean places of 404 stars within two degrees of the pole is nearly completed. The work was pressed during the last quarter, as Professor Whitney then secured the services of an expert computer. The intercomparison of the plates and the determination of proper motion remains to be studied.

BIBLIOGRAPHY.

Robert Fletcher, Army Medical Museum, Washington, D. C. Grant No. 30. *For preparing and publishing the Index Medicus.*
$10,000.

The Index Medicus was established in 1879, under the direction of Dr. John S. Billings and Dr. Robert Fletcher, and discontinued in 1899, after 21 volumes had appeared, for the lack of pecuniary support.

Abstract of Report.—The scope of this work is very broad with relation to the medical sciences. It contains, in classified form, month by month, reference to everything published throughout the world which relates to medicine or public hygiene. The latter comprises

all that concerns the public health in its municipal, national, and international relations.

Nine numbers of the volume have been issued, and the volume will be complete with the January number, when the "Annual Index" will be compiled. The Index is a very elaborate piece of work, and will comprise 200 pages in double or triple columns. The work is of great value to all the medical profession, especially to professors in medical schools and colleges, officers of health, and workers in scientific laboratories.

The subscribers to the Index Medicus are chiefly residents of the United States, but the list includes subscribers in England, Ireland, Scotland, Canada, Australia, France, Germany, Spain, Portugal, Roumania, Sweden, Switzerland, and Manila. There are now 455 subscribers.

Herbert Putnam, Washington, D. C. Grant No. 56. *For preparing and publishing a Handbook of Learned Societies.* $5,000.

In order that the scientific investigators of this country, and especially those connected with the Carnegie Institution, might have an accurate knowledge of the agencies which now exist for the promotion of scientific inquiry in every part of the world, the Advisory Committee on Bibliography recommended that a descriptive catalogue be prepared of all the learned societies of the world.

At the present time such information, and particularly regarding the publications of learned societies, is incomplete and unorganized, being scattered through a large and miscellaneous collection of volumes, many of which are inaccessible and not well known. A careful and comprehensive list would be of great value to all the librarians of the country who aim at the preservation of the transactions of learned bodies. It would also furnish a basis for exchanges. The funds for research work held by these various institutions have special significance with reference to the activities of the Carnegie Institution. The plan of the Handbook included information as to these eleven points:

(1) Name or names of the society or institution, indicating any change which may have occurred, with cross references.

(2) Objects of the society.

(3) Brief historical note.

(4) Endowments, research funds, prizes, etc.

(5) Offices of the society.

(6) Membership, numbers, conditions and manner of election, dues, etc.

(7) Meetings—their character, frequency, time, and place.

(8) Communications—regulations for presentation and publication of papers.

(9) List of officers, with address of corresponding secretary.

(10) Complete and detailed bibliography of all regular or special publications since the foundation of the society, editions (how large?) to satisfy all the above mentioned requirements.

(11) Publications—conditions and methods of distribution; prices.

According to the plan of work approved, the handbook is to be in volumes; societies to be classified by subjects, with local arrangement, and each class to constitute a separate part. The following order of procedure has been adopted:

(*a*) To prepare a list of societies from the exchange lists at the Smithsonian Institution and elsewhere in Washington, and a card catalogue to keep orderly record of communications.

(*b*) To issue a suitable circular to these societies, requesting the desired information.

(*c*) To prepare for publication the material received, filling out lacunæ by further correspondence and reference to various sources of information.

(*d*) In the case of societies not replying to circular or letter, and in regard to which sufficient information cannot be obtained from printed sources, to adopt such other methods as the progress of the work may suggest.

The first stage of this work was the preparing of a card catalogue of names of learned societies and institutions. Every source of information known and available in the Congressional Library was searched to make this as nearly complete as possible, at the same time separating (1) dead societies and (2) societies not publishing any material of importance to investigators.

The second stage of the work was the sending of a circular letter, containing an outline of the information required, to academies and societies dealing with historical and social science in Europe and North America. Russia and other Slavic countries, and also Austria and Hungary, are being treated independently, advantage being taken of a visit to Russia by Mr. A. V. Babine, of the Library of Congress. Mr. Thompson and Mrs. Thompson made personal visits to England, Paris, Belgium, Holland, and Berlin for the purpose of

supplementing the information obtained by correspondence. It is anticipated that Mr. Thompson will also visit Italy and Switzerland.

The third stage of the work, the reduction of the replies received to standard form, was begun in August, and is now going on in the office at Washington. It is expected that this work will be brought to completion in 1904.

BOTANY.

W. A. Cannon, New York Botanical Garden, N. Y. Grant No. 27. *For investigation of plant hybrids.* $500.

Abstract of Report.—Under this grant Mr. Cannon worked at the New York Botanical Garden until September 1, 1903. He prepared a paper on the spermatogenesis of the hybrid peas* and collected material for the study of the sporogenesis of two fern hybrids.

H. S. Conard, University of Pennsylvania, Philadelphia. Grant No. 8. *For study of types of waterlilies in European herbaria.* $300.

Abstract of Report.—The grant made to Mr. Conard was to enable him to examine the types of waterlilies in various European herbaria for the purpose of completing a memoir on waterlilies which the Carnegie Institution is about to publish. He was successful in obtaining the requisite data, and the memoir will soon go to press.

Desert Botanical Laboratory (F. V. Coville and D. T. MacDougal, Washington, D. C.). Grant No. 26. $8,000.

At the meeting of the Trustees in November, 1902, a comprehensive plan for the encouragement of botanical researches was submitted by the Advisory Committee on Botany (see Year Book, No. 1, pages 3–12).

In carrying out this plan, Mr. F. V. Coville, Botanist of the Department of Agriculture, Washington, and Mr. D. T. MacDougal, Director of the Laboratories of the New York Botanical Garden, were requested to go to the arid lands of the west and make such further recommendations as might seem to them best. They became persuaded that the best position for the laboratory, considering both natural and artificial advantages, is Tucson, Arizona, and they recommended its establishment there and the engagement of Dr. W. A. Cannon to be resident investigator.

* Contributions of New York Botanical Garden, No. 45, Bull. Torrey Bot. Club, Oct. 30, 1903.

A full report with respect to the organization of this laboratory and of the various circumstances which led up to it will be published in a monograph soon to be printed among the publications of the Carnegie Institution.

Abstract of Report.—Messrs. Coville and MacDougal were appointed a committee on the subject of a Desert Botanical Laboratory.

After their visit to the principal points in the southwestern desert region, a laboratory location was selected near Tucson, Arizona.

The building site, water supply, road, and electrical connection were presented by the Chamber of Commerce of Tucson, the cash value of these concessions amounting to about $1,400, and the discussions that took place initiating what is still more valuable—the hearty interest and coöperation of the citizens in the purposes of the laboratory.

A laboratory building has been planned, contracted for, and completed, the contract price being $3,843. The laboratory has been equipped with books, apparatus, furniture, and supplies, at a cost of $1,813.50.

Dr. W. A. Cannon, recently connected with the New York Botanical Garden (Bronx park), New York, was appointed resident investigator, and took charge of the laboratory September 1. He is now engaged in investigating the root systems of desert plants with reference to their special devices for the absorption and storage of water.

The privileges of the laboratory have been granted to Professor Charles B. Davenport, University of Chicago, for an inquiry into the morphological and physiological adjustments of desert animals to their habitat. Other applications are pending.

The committee has presented an illustrated report on the laboratory location, which is now in press as a publication of the Institution.

E. W. Olive, Crawfordsville, Ind. Grant No. 32. *Researches on the cytological relations of the Amœbæ, Acrasiæ, and Myxomycetes.* $1,000.

Abstract of Report.—Mr. Olive's work has been carried on in Professor Strasburger's laboratory in the Botanical Institute at Bonn, Germany. In order to do this work he resigned his position as instructor at Harvard University. His studies include cultures of the Acrasiæ and of the Labyrinthuleæ, which he had brought from America.

Mr. Olive's report shows definite progress in his research, and the prospect of the completion within two months of two papers incorporating a portion of his results.

Janet Perkins, working at the Royal Botanical Gardens, Berlin, Germany. Grant No. 19. *For preliminary studies on the Philippine flora.* $1,900.

Abstract of Report.— Dr. Janet Perkins reports that she was engaged in the proposed investigation from February 20 to October 5, 1903. A catalogue of the Philippine flora was begun, based on various monographs and papers which have appeared in scientific periodicals. This work consumed much time, as literature regarding the Philippines is greatly scattered, and the synonymy needs a thorough clearing up.

Among other matters that were begun were:

(*a*) A catalogue of the various native names.

(*b*) A list of botanical literature pertaining to the Philippines.

(*c*) The attempt to construct a type herbarium of Philippine plants.

(*d*) The determination of certain Philippine plants received from the Department of Agriculture.

(*e*) The preparation of a sample copy of the manuscript and illustrations for the position of the family Marantaceæ.

CHEMISTRY.

John J. Abel, Johns Hopkins University, Baltimore, Md. Grant No. 24. *For study of the chemical composition of the supra-renal gland.* $1,000.

Abstract of Report.—The vital importance of the supra-renal glands was first pointed out a half century ago. Repeated efforts to explain why these organs are physiologically necessary were of no avail until 1895, when it was discovered that the juice expressed from these glands contains a remarkable principle found nowhere else in the human body.

This important substance has the power to raise the blood-pressure of man and other animals when given in very minute amounts,

and this fact has been made the basis of a theory which seeks to explain the importance of these glands, which alone secrete it.

It is evident that but little scientific or practical progress could be made until this active principle should be separated from the innumerable other constituents of the gland. This isolation was first effected by Dr. Abel in 1897, and he named the product obtained by him Epinephrin.

Investigators are making every effort to elucidate the chemical constitution of this compound, not only with a view to tracing its origin and its fate in the animal body, but also looking to its possible preparation by synthetic methods—that is, by chemical art, without the intervention of the animal organism.

Progress of the most promising character has recently been made in Dr. Abel's laboratory. The work is being actively prosecuted and will be continued throughout the present winter.

Since receiving a grant from the Carnegie Institution, Dr. Abel has published three papers on the subject, and a fourth communication bearing on the true composition of epinephrin and epinephrin hydrate and their chemical constitution will shortly make its appearance.

W. D. **Bancroft**, Cornell University, Ithaca, N. Y. Grant No. 6. *For a systematic chemical study of alloys, beginning with the bronzes and brasses.* $500.

Abstract of Report.—The experimental work under this grant has been done by Mr. E. S. Shepherd, under the direction of Professor Bancroft. They have analyzed the different solid bases and determined the copper-tin-lead diagram except for the alloys containing less than 20 per cent of copper. They have determined the densities and electro-motive forces of the annealed bronze, and made a careful microscopic study of the same alloys. Work is now under way on the density and determination of bronzes cast in vacuo, the copper-tin-lead diagram, and the making of the necessary analyses. A study of the physical properties of bronzes will be carried on during the winter.

L. M. Dennis, Cornell University, Ithaca, N. Y. Grant No. 42.
For investigation of the rare earths. $1,000.

Professor Dennis has been engaged for the past ten years in the study of the rare earths, and has accumulated a large amount of purified material. He proposed to carry on a study with special reference to improvements in the methods for determining the atomic masses of these substances, and for separating the elements of the yttrium group.

Abstract of Report.—The work under this grant was carried on by Dr. Benton Dales in the laboratory of Professor Dennis, of Cornell University.

Dr. Dales has submitted a report on the ammonium carbonate and acetic acid method of fractionation. The source of the rare earths used in the work was xenotime, essentially a phosphate of the yttrium group of earths from Brazil. The work is unfinished, owing to Dr. Dales having resigned his position at Cornell University before completing it. Three-fourths of the grant was used. A paper containing the results of the investigation, as far as obtained, was transmitted for publication.

H. C. Jones, Johns Hopkins University, Baltimore, Md. Grant No. 39. *For investigations in physical chemistry.* $1,000.

Abstract of Report.—Under the direction of Professor Jones, Dr. F. H. Gatman began work October 1, 1903, by investigating certain apparently abnormal phenomena manifested by concentrated solutions of electrolytes in water and other solvents. They expect to be able to report considerable progress by the end of the year.

H. N. Morse, Johns Hopkins University, Baltimore, Md. Grant No. 34. *For researches on osmotic pressure.* $1,500.

Abstract of Report.—Professor Morse reports that the immediate problem to be solved was the development of a practical method for measuring osmotic pressure. Although osmotic pressure has been recognized for twenty five years as one of the great forces of nature, there have been no direct measurements to furnish an adequate experimental basis for the laws supposed to govern it. Professor Morse has been engaged for several years in attempting to overcome the difficulties which lie in the way of quantitative measurements of osmotic pressure. He states the problem under three heads, as follows:

1. The preparation of a suitable semipermeable membrane.

2. The overcoming of the mechanical difficulties in assembling the different parts essential to the complete osmotic cell.

3. The production of an efficient porous wall on which to deposit the semipermeable membrane.

Professor Morse has succeeded in solving the problems designated by 1 and 2, and the work since October, 1902, has been prosecuted by him and Mr. J.C.W. Fraser, working in the laboratory of the Johns Hopkins University. They have found it necessary not only to work out theoretically but also practically the problem of the production of a suitable porous wall, necessitating the molding of the clay under great pressure in order to give the cell wall a higher and more uniform degree of compactness than is secured by the usual methods of the potter, and to remove thoroughly the air blisters and cavities which render most porous walls unfit for experimental work in osmotic pressure. Their attention was therefore turned, in the second place, to the devising of apparatus for the forming of the clay vessels under pressure, with the result that they now possess two pieces of apparatus which work to entire satisfaction. They next proceeded to take up the problem of baking the clay vessels, and devised an electric kiln which was effective and well adapted to general use in the laboratory. They are now ready to begin the making, baking, and burning of porous cells.

A. A. Noyes, Massachusetts Institute of Technology, Boston, Mass.
Grant No. 45. *For certain chemical investigations.* \$2,000.

Abstract of Report.—The work under the direction of Professor Noyes, on the electric conductivity of salts and aqueous solutions at high temperatures, has been in progress for several months, with the assistance of Dr. William D. Coolidge. Much of the time has been given to the construction of an effective platinum lined conductivity cell or bomb, suitable for exact conductivity measurements with aqueous solutions up to 306° or higher, and in other preparatory work.

Now that the serious difficulties in the production of the conductivity apparatus, suitable for measurements at high temperatures and pressures, have been overcome, and the possibility of obtaining accurate results has been demonstrated by a series of determinations extending with a few salts up to 306°, it is highly desirable to extend the measurements to salts of other types and to acids and bases, and to the critical temperature of 360°. This work is very difficult,

and it will be necessary to continue it for a number of years before it will be completed.

Two other researches for which the aid granted was employed were begun in September, with the assistance of Dr. Herman C. Cooper and Mr. Yogoro Kato.

Theo. W. Richards, Harvard University. Grant No. 41. *For investigation of values of atomic weights, etc.* $2,500.

Abstract of Report.—Professor Richards has submitted a memoir, about to be published by the Carnegie Institution, containing the records of his experiments on a new method of determining compressibility. By means of this method the compressibilities of bromine, iodine, chloroform, bromoform, carbon tetrachloride, phosphorus, water, and glass have been determined over a range of 700 atmospheres.

Besides the continuation of the preceding work, several other investigations are in progress, assisted by this grant. One of these concerns the effect of pressure on the electrochemical solution tension of metals; another concerns the heat capacity of solutions; and another concerns the atomic weight of sodium.

J. Bishop Tingle, Illinois College, Jacksonville, Ill. Grant No. 40. *For continuing investigations on the derivatives of camphor and allied bodies.* $500.

Abstract of Report.—The work under this grant was not begun till late in the summer. A number of bases have been tested as to their power to undergo condensation with camphoroxalic acid and its ethylic salt. Experiments have also been made to obtain further information as to the possible presence of hydroxyl groups in camphoroxalic acid, with encouraging results.

Engineering.

W. F. Durand, Cornell University, Ithaca, N. Y. Grant No. 64. *For experiments on ship resistance and propulsion.* $4,120.

Abstract of Report.—Professor Durand reports that certain equipment necessary for the conduct of the experiments was completed early in the spring. Experiments in connection with the work on propellers was begun, and all of the work of observation required for the complete determination of the performance of thirty-five

model propellers was finished. To complete the investigation immediately in view, fourteen propellers remain to be experimented with. He feels that the complete experimental determination for thirty-five propellers constitutes a most satisfactory summer's work. This is five-sevenths of the entire field to be covered by this particular investigation. The work of making the detailed reductions and analyses of these observations will presumably occupy most of the winter. But very gratifying progress has been made in the preliminary measurements, speed having been determined from distance and time records in 444 cases and thrust-turning momentum determined by integration from autographic records in 655 cases.

Leonard Waldo, New York city. Grant No. 22. *For study of aluminum bronzes.* $4,500.

Abstract of Report.—Mr. Waldo reports that through the death of his associate, George S. Morison, and the breakdown in health of his chief assistant progress has been slow; he is unable to do more than report progress. He

(*a*) Prepared a bibliography on alloys of aluminum and copper and of other aluminum compounds.

(*b*) Has had in operation six kinds of specially built furnaces, and is building a seventh, to determine the best methods for making large castings and sound wire bars or billets of aluminum bronze.

(*c*) His rolling mill experiments for producing tubes, sheet, wire, and forged bars, from billets cast during the year, are practically complete and are satisfactory.

Notes taken during the process of rolling and cold drawing, relative to temperature, speeds, and cost are awaiting collation and reduction. A complete report will be prepared during the coming year.

EXPLORATION.

Raphael Pumpelly, Newport, R. I. Grant No. 37. *For preliminary examination of the trans-Caspian region.* $6,500.

Abstract of Report.—The reconnaissance covered a region of 1,750 miles in length, with trips from 10 to 300 miles away from the railroad base. Throughout the great part of this area the remains of ancient occupation abound, in the form of large tumuli, village sites, fortresses, and cities.

The structure of the tumuli examined and their contents indicate a very remote beginning and occupation during long periods. The

builders had apparently archaic pottery, no metals, slight knowledge of stone implements, and probably wooden weapons. The people were settled and had the domestic horse, cow, pig, sheep, and goat. Many of these seats of early dwelling seem to have become in time eminences upon which arose fortresses, or to have become the citadels of towns growing up around them. Thus they probably contain the continuous record of the development of the civilizations of the region from a very remote antiquity down to historic times.

The reconnaissance work of Professor Davis, Mr. Huntington, and R. W. Pumpelly has shown the former existence of several glacial epochs, and has made much progress in correlating these with the progress of prehistoric physical events in the building of the plains and the expansions of the former Aralo-Caspian seas. Their observations give reason to hope that further study will correlate these physical events with important phases of human development in connection with Asiatic and European history.

GEOPHYSICS.

Frank D. Adams, McGill University, Montreal, Can. Grant No. 4.
For investigating the flow of rocks. $2,500.

Professor Adams has been engaged for some years past in an experimental investigation into the nature of the movements set up during the folding and deformation of the rocks of the earth's crust.

Abstract of Report.—Dr. Adams reports that McGill University has provided for his use in carrying on the investigation on the flow of rocks a large room in the basement of the new chemical building of the University. In this room he has installed the apparatus he formerly had and ordered a third and much more powerful hydraulic press, by which pressure up to 120 tons may be secured and maintained, if necessary, for weeks at a time. Ample provision has been made in the installation of the new hydraulic press, looking to the possibility of the extension of the plant in its adaptation to the most varied experimental uses.

On the completion of the installation Dr. Adams commenced the investigation of high differential pressures on dolomites from Maryland, Massachusetts, and the Province of Quebec. It was found that at ordinary temperatures these dolomites could be made to flow in the same manner as in the case of the pure Carrara marble. He is now carrying on experiments to ascertain the effect of heat upon the flow of dolomite. In order to compare the effects produced at high

pressures with those produced by lower pressures, the higher representing the condition at lower depths in the earth's crust, experiments have been begun on the flow of marble with the 120-ton press.

Dr. Adams is also carrying on a series of investigations into the force required to drive water through Portland oölite, which is the rock he has selected for further experiments on the deformation of limestones when heated, with water passing through them. He has also assembled material to commence the study of granite essexite and diabase, as typical igneous rocks under very high pressures at ordinary temperatures.

C. R. Van Hise, University of Wisconsin, Madison, Wis. Grant No. 71. *For investigating the subject of geophysical research, etc.* $2,500.

In the Year Book for 1902, page 26, an extended report was presented on the subject of geophysics. As the trustees were not prepared to act upon the project, a further study of the problem was made, at the request of the Executive Committee, by Professor Van Hise, who investigated the subject of geophysical research in European institutions and made a report, which is printed in the Accompanying Papers of this report, pages 173–184.

GEOLOGY.

T. C. Chamberlin, University of Chicago, Chicago, Ill. Grant No. 31. *For study of the fundamental principles of geology.* $6,000.

Abstract of Report.—Plans for the consideration of the different phases of the complex subjects of this investigation were arranged with numerous collaborators, and details of this collaboration and the results obtained are given in Professor Chamberlin's report, printed in the Accompanying Papers in this volume, pages 261–270.

Bailey Willis, U. S. Geological Survey, Washington, D. C. Grant No. 72. *For geological exploration in eastern China.* $12,000.

This grant was for the purpose of carrying on a comparative study of the geology of eastern Asia and western North America, by observations in stratigraphy, structure, and physiography in eastern China and Siberia, and by the collection of fossils, particularly with reference to the development of the Cambrian faunas.

He proposed to begin his inquiries in the mountain district in Shantung—the Tai-shan—a geological unit of about 4,000 square miles, where a study could be made of the geology from pre-Cambrian gneisses to the Coal Measures.

Mr. Eliot Blackwelder, an instructor in elementary geology and paleontology in the University of Chicago, accompanied Mr. Willis.

Abstract of Report.—Under date of September 30, 1903, from Tientsin, China, Mr. Willis reports that all preparations are completed, that authority has been received from the Chinese and German governments, and that with his associate, Mr. Blackwelder, he is about to leave for the province of Shantung. From Shantung it is proposed to go to Liautung.

Mr. Willis expects to return to Pekin January 1, 1904, and as soon as may be thereafter to enter upon a trip that will probably continue until the end of June, 1904.

History.

Worthington C. Ford, Washington, D. C. Grant No. 28. *For an examination of the historical archives of Washington.* $2,000.

For the purpose of studying the historical archives of Washington and ascertaining their extent and their characteristics, Mr. Ford prepared a scheme of inquiry which was arranged in two divisions. The first division included a general statement of the contents of each repository of archives, a statement of the place in which it is contained, and the history of the collection; also a statement of the funds available for the maintenance of the collection and of the conditions under which documents are accessible. The second division referred to the preservation of the collections and the arrangements for enlarging them.

Abstract of Report.—The purpose of this grant was to defray the expense of making a general survey of the archives of the government and the preparation of a report which would be helpful to historical investigators. Dr. Claude H. Van Tyne and Mr. Waldo G. Leland began the work in January, 1903, following general suggestions offered by Mr. Ford. They have examined the manuscript material in every branch of the government, and have prepared a statement as to the nature and extent of the administrative records, as well as of the more important collections of historical material. This description is now nearly ready for printing. It will make a

book of 250 or 300 pages of the size of the Year Book. While it does not attempt to describe individual documents, but only classes and collections of documents, it is sure to be helpful to historical scholars seeking material.

PALEONTOLOGY.

E. C. Case, State Normal School, Milwaukee, Wis. Grant No. 46. *For continuation of work on the morphology of Permian reptiles.* $500.

Abstract of Report.—In connection with the preparation of a monograph on the Pelycosauria of the American Permian deposits, Professor Case spent most of the summer in the British Museum and several weeks in the museums of Paris and Berlin in the study of the reptiles of Permian age contained therein. The main line of work resolved itself into a careful comparison of the faunas of the deposits of America, Russia, and South Africa. The most important result was the demonstration that American forms are practically completely different from those of Russia and South Africa, the sole connecting faunas being of the most primitive type and none, so far as known, being common. This emphasizes the peculiarity of the presence of a typical American Pelycosaurian in the deposits of Bohemia. Professor Case also obtained many isolated facts of morphology that will be of material assistance to him in the study of the fauna.

O. P. Hay, American Museum of Natural History. Grant No. 14. *For monographing the fossil Chelonia of North America.* $2,200.

Abstract of Report.—Dr. Hay reports that he has prepared 200 pages of typewritten manuscript, and has had made, under his personal supervision, 210 drawings and 80 photographs of fossil turtles. He finds that there are about 180 species, and that there yet remains much to be done before the monograph will be ready for publication. During the summer he spent two months in the Bridger deposits of Wyoming, collecting fossils, and secured 135 specimens of turtles that will add greatly to our knowledge of Eocene forms.

G. R. Wieland, Yale University, New Haven, Conn. Grant No. 48. *For continuation of his researches on living and fossil cycads.* $1,500.

Abstract of Report.—Dr. Wieland expects to have a memoir ready by the close of the calendar year, dealing with the fossil cycads from

a biological standpoint. He has developed a new method for the study of fossil cycads by perfecting or inventing inverted drills, by means of which he has secured leaves, branches, fruits, flowers, and terminal buds in the form of cylindrical cores cut from the cycad trunks. He has also adopted the novel plan of cementing together again, in their original position, the parts of such cores resulting from the cutting of a series of thin sections, and in this way securing a second series, also complete. By these methods he has cut a dozen fruits, in various stages of growth, from a silicified cycad trunk. He has also cut thin longitudinal and transverse sections of flowers surrounded by leaf bases. It is now possible to make, in the case of cycads, intensive studies of single trunks, such as have never before been made in the case of any fossil plants.

S. W. Williston, University of Chicago, Chicago, Ill. Grant No. 49. *For preparing a monograph on the Plesiosaurian group.* $800.

Abstract of Report.—Professor Williston reports that he investigated the type material of Plesiosaurs at Colorado College, University of Kansas Museum, the American Museum of Natural History in New York, the Museum of the Academy of Natural Sciences, Philadelphia, and the National Museum, Washington. Important material has been sent him from these and other sources, upon which he is at present engaged. He hopes to complete his study during the year 1904.

Physics.

Henry Crew, Evanston, Ill. Grant No. 10. *For study of certain arc spectra.* $1,000.

Abstract of Report.—Professor Crew reports that after the building of certain apparatus, which required several months, he began the experimental part of his work. He found unexpected difficulties in working with magnesium and zinc, the two metals in which he hoped to find the order of appearance of the lines of the spark spectra.

His second problem was to complete the maps of the spectra of cadmium and aluminum. The map of the cadmium arc has been completed; that of aluminum nearly so.

The difficulty of obtaining an oscillograph has delayed the beginning of work on the third problem, the determination of the E. M. F. curve with the "rotating metallic arc."

A. A. Michelson, University of Chicago, Ill. Grant No. 47. *For aid in ruling diffraction gratings.* $1,500.

Abstract of Report.—Professor Michelson encountered many serious difficulties in the ruling engines for diffraction gratings, most of which he now believes are overcome. The work is now being pushed vigorously, and he hopes before another year to make a favorable report on the results obtained.

Harold Pender, Johns Hopkins University, Baltimore, Md. Grant No. 18. *For experiments on the magnetic effect of electrical convection.* $750.

Abstract of Report.—The object of Dr. Pender's grant was to perform in Paris, in conjunction with Mons. B. Cremieu, experiments on the magnetic effect of electrical convection and to confer with M. Poincaré concerning the same. Dr. Pender met with great success in clearing up a controverted question as to the presence of a magnetic field about a bare metallic surface when charged and set in motion, which field is in all probability due to what is usually termed a convection current of electricity.

R. W. Wood, Johns Hopkins University, Baltimore, Md. Grant No. 25. *For research, chiefly on the theory of light.* $1,000.

Abstract of Report.—Professor Wood reports that one half of the grant has been expended for the salary of an assistant, and that the balance he plans to expend for apparatus. Through the aid given he was able to accomplish much more experimental work than he otherwise could have done. During the year he obtained results which were published in seven papers, all of which pertain to researches connected with the theory of light.

A considerable amount of work was also done on an investigation on the dispersion of sodium vapor; this has not yet been published.

PHYSIOLOGY.

W. O. Atwater, Wesleyan University, Middletown, Conn. Grant No. 5. *For experiments in nutrition.* $5,000.

Abstract of Report.—The purpose of this grant was to promote researches involving the direct determination of the amount of oxygen consumed by man for sustaining the bodily functions. The grant has been expended chiefly for the services of experts and assistants,

for devising and constructing or purchasing apparatus, for developing methods for the determination of oxygen, and for efficiency tests and experiments with men in the apparatus.

Several tests of the efficiency of the apparatus and method of manipulation were made. The feasibility of the use of the apparatus for the experiments with men has also been tested by three experiments with different subjects, with satisfactory results. Attention is now being devoted to alterations and improvements in the apparatus and to modifications of methods; efficiency tests and experiments with men are also in progress.

Arthur Gamgee, Montreux, Switzerland. Grant No. 62. *For preparing report on the physiology of nutrition.* $6,500.

Abstract of Report.—Dr. Gamgee began and has carried on a study of the extensive literature on this subject, which had to be mastered for the purpose of the inquiry on which he was engaged. He began by inspecting European laboratories and by visiting scientific men in Europe. He also visited Professor Atwater, at Middletown, Conn., and acquainted himself with the work now in progress there. He also visited other Americans. It is probable that his complete report will be transmitted in May, 1904.

Psychology.

G. Stanley Hall, Clark University, Worcester, Mass. Grant No. 61. *For certain investigations on the anthropology of childhood.* $2,000.

Abstract of Report.—The result of Dr. Hall's work in connection with this grant is best indicated by the titles of the papers he has published, giving the results obtained during the year. These are:

1. Reaction to light and darkness.
2. Children's ideas of fire, heat, frost, and cold.
3. Curiosity and interest.
4. Showing off and bashfulness as phases of self consciousness.
5. Marriage and fecundity of college men and women.

E. W. Scripture, Yale University, New Haven, Conn. Grant No. 21. *For researches in experimental phonetics.* $1,600.

Report.—Professor Scripture's report is printed in the Accompanying Papers in this volume, pp. 243–259.

ZOÖLOGY.

H. E. Crampton, Columbia University, New York. Grant No. 9. *For determining the laws of variation and inheritance of certain lepidoptera.* $250.

Abstract of Report.—In order to obtain data for the problems of variation, their relation to selection, and for the study of correlation, Dr. Crampton investigated the following material:

(*a*) Eight hundred and forty-eight cocoons of *Philosamiacy nthia.*
(*b*) Fourteen hundred and ten cocoons of *Samia cecropia.*
(*c*) Four hundred cocoons of *Callosamia angulifera*, etc.
(*d*) Seventy-five cocoons (preliminary) of *Attacus orizaba.*
(*e*) One family, *Hypercheiria io.*

The data secured furnish material for examination into variation and selection by comparing—

(*a*) Metamorphosing and non-metamorphosing.
(*b*) The perfect and imperfect survivors.
(*c*) The mating and non-mating moths.

Dr. Crampton thinks that certain general conclusions are justified from the facts already determined. Surviving individuals are less variable than those which succumb; mating individuals are less variable than those which fail to mate; and the index of correlation of the pupal characters is higher for the selected individuals in both cases. In a word, selection proceeds upon a basis of deviations from type and upon a correlative basis.

J. E. Duerden, Chapel Hill, N. C. Grant No. 12. *For investigation of recent and fossil corals.* $1,000.

Abstract of Report.—With a view to obtaining suitable material for continuing his researches on fossil corals, Dr. Duerden has lately visited the principal museums and geological surveys in Great Britain, where Paleozoic corals are most abundant. These museums, and also the Smithsonian Institution, have placed at his disposal numerous specimens. Other material has been purchased. These collections will be studied during the present winter, with the hope of showing the relationship of fossil to recent corals.

Dr. Duerden has deposited with the Carnegie Institution, with a view to its publication, the manuscript and drawings of a memoir entitled "The coral *Siderastræa radians* and its post-larval development." This work is illustrated by fifteen plates and numerous text

figures and gives an account of the morphology of a coral and its growth for a period of four months. It carries the development of the coral much farther than any previous work and contains many fundamental results in madreporarian morphology.

C. H. Eigenmann, Indiana University, Bloomington, Ind. Grant No. 68. *For investigating the blind fishes of Cuba.* $1,000.

Abstract of Report.—Dr. Eigenmann did not begin his work under the Carnegie grant until October. He expects to spend from four to six months in Cuba, during the entire breeding season, and to make general collections in the caves and streams. He will also make an effort to secure the blind fishes from the island of Jamaica. He has made arrangements with the Cuban government to coöperate with him, as far as practicable, in giving him facilities for carrying forward his investigation.

L. O. Howard, Department of Agriculture, Washington, D. C. Grant No. 38. *For preparing manuscript and illustrations for a monograph on American mosquitoes.* $2,000.

Abstract of Report.—Dr. Howard began his work by making arrangements to secure observers at points in the United States, Central America, and the West Indies sufficiently different in their faunistic characteristics to promise comparatively little duplication. He also published an announcement of the proposed monograph for the purpose of attracting volunteer observers and contributors; and, through correspondence, a great deal has been done in that direction, both in the West Indies and the United States. He also utilized the services of a number of the members of his force in the Department of Agriculture in making collections and observations.

He reports that the results as a whole have been surprising to him. A number of new species of mosquitoes have been discovered and one new genus, and much important specific information regarding the geographic distribution of the different species has been gained. This information has been of special interest and value regarding the yellow fever mosquito (*Stegomya fasciata*) and the different species of the malaria bearing mosquitoes of the genus Anopheles. A new species of this genus was found in the immediate vicinity of Washington. Great advance has been made in following out the life histories of the different species and genera; this has been done for nearly one hundred species.

All the collections and specimens have not yet been received by Dr. Howard, but every observer will send a series of specimens of adults, eggs, larvæ, and pupæ, together with cast larval skins of all species observed. These have been and will be accompanied by full notes of habits, etc., together with drawings of structural peculiarities.

H. S. Jennings, University of Michigan, Ann Arbor, Mich. Grant No. 15. *For experiments on the behavior of lower animals.* $250.

Abstract of Report.—Dr. Jennings, who is a research assistant of the Carnegie Institution, is now at the Marine Biological Laboratory at Naples, carrying forward investigations on the reactions and behavior of very low organisms, such as Amœba and other Rhizopoda. He expects to have a general work in regard to the behavior of the lowest organisms ready for publication during the year. He has submitted to the Institution for publication a paper entitled "Reactions to heat, light, and other stimuli in the ciliate infusoria and in Rotifera, with considerations on the theories of animal behavior."

C. E. McClung, Kansas University, Lawrence, Kans. Grant No. 16. *To making a comparative study of the spermatogenesis of insects and other classes of arthropods, and if possible to determine the specific functions of the different chromosomes.* $500.

Abstract of Report.—Professor McClung reports that owing to the fact that his own work and that of others show the main features of insect spermatogenesis, he determined to make use of the grant for the prosecution of other more difficult and expensive studies. He commenced by purchasing some literature to which he did not have access, and began the search for an object upon which he might prosecute his investigations. There appeared to be two ways to get at the problem—to study the germ cells of hybrids or to experiment upon fertilized eggs in the early cleavage stages. He decided to adopt the first mentioned plan for the beginning of the work. With this object in view, he spent the summer at the Woods Hole Marine Biological Laboratory, but did not succeed in obtaining satisfactory forms of hybrids. He feels certain, however, that if the proper animals are secured the true function of the chromosomes may be settled as definitely as any other fact relating to cell structure.

E. B. Wilson, Columbia University, New York. Grant No. 36. *For investigations in experimental embryology, etc., in Naples.* $1,000.

Abstract of Report.—Dr. Wilson utilized this grant to defray the expenses of a visit to the Naples Zoölogical Station, extending from February to July, during which time he was actively engaged on studies in experimental embryology. His first purpose was to search for available material for the experimental analysis of the early developmental stages in mollusks and annelids, which possess high theoretical interest in their bearings on the general problems of differentiation. He reports a large measure of success in this direction. He found two excellent objects for his research, and made as exhaustive an analysis of them as the time would permit. He demonstrated conclusively the mosaic character of the development in the molluscan egg, and obtained striking evidence of the self differentiation and specification of embryonic cells. This result is interesting from its bearing on the problem of differentiation and also, perhaps, in even a greater degree, through the firm basis which it gives for the general method and point of view in studies of cellular embryology.

A second general division of his work included the experimental study of prelocalization in the unsegmented egg, which yielded results of no less interest than the cleavage stages. Of these the most important relate to the embryonic basis of correlation and to the relation between quantitative and qualitative prelocalization in the germ.

Dr. Wilson adds a general comment on the nature of this work to the effect that its principal significance lies in its connection with recent studies of the cellular basis of inheritance and development, taken in connection with experimental studies of heredity such as those that have grown out of the rediscovery of the Mendelian law. He is fully persuaded that there is now a very good prospect of making an essential advance toward an understanding of the actual mechanism of hereditary transmission, and expresses the hope that the studies in this direction may receive their due share of support.

H. V. Wilson, University of North Carolina, Chapel Hill. Grant No. 33. *For morphology and classification of deep sea sponges.* $1,000.

Abstract of Report.—In order to complete his investigation of the deep sea sponges of the Pacific ocean, Professor Wilson visited the

museums of London, Paris, Leiden, and Berlin to make a direct examination of the types stored therein. He returned to America in August, and is at present engaged upon the text of his report.

Marine Biological Laboratory, Woods Hole, Mass.; J. Blakely Hoar, treasurer. Grant No. 35. *For maintenance of 20 tables*, $10,000.

Abstract of Report.—This appropriation was made for the purpose of aiding the laboratory by paying for the maintenance of twenty research tables. The persons assigned to the tables were selected by the Carnegie Institution.

The following investigators occupied the Carnegie tables during the season of 1903:

Names.	Dates.
1. Prof. M. A. Bigelow Columbia University, N. Y.	May 30 to Aug. 8.
2. Dr. R. M. Strong University of Chicago, Ill.	June 16 to Sept. 18.
3. Prof. C. E. McClung University of Kansas, Lawrence.	July 6 to Aug. 24.
4. Prof. George Lefevre University of Missouri, Columbia.	July 28 to Aug. 19.
5. Prof. Wm. E. Kellicott Barnard College, N. Y.	June to August.
6. Prof. Arthur W. Greeley Washington University, St. Louis.	June 25 to Aug. 15.
7. Mr. C. J. Brues Columbia University, N. Y.	June 30 to Sept. 1.
8. Mr. Fred. E. Pomeroy Bates College, Lewiston, Me.	June to August.
9. Mr. J. W. Scott University of Chicago, Ill.	June 22 to Aug. 24.
10. Dr. H. G. Spaulding College of the City of New York.	June to August.
11. Dr. Leo Loeb McGill University, Montreal, Canada.	July 11 to Sept. 5.
12. Dr. Henry Kraemer Philadelphia, Pa.	July 13 to Aug. 10.
13. Mr. Grant Smith Harvard University, Cambridge, Mass.	July to August.
14. Prof. Joseph Guthrie Iowa State College, Ames, Iowa.	July to August.
15. Miss A. B. Townsend Cornell University, Ithaca, N. Y.	July to August.
16. Mr. M. A. Chrysler University of Chicago, Ill.	July to August.

17. Mr. Gustav Ruediger . . . . July 7 to Aug. 17.
    Chicago, Ill.
18. Miss Helen Dean King . . . . June 10 to July 13.
    Bryn Mawr University, Pa.
19. Mr. James A. Nelson . . . . July to August.
    University of Pennsylvania.
20. Prof. Christian P. Lommen . . . . . . July to August.
    University of South Dakota.

The Director of the Laboratory, Dr. C. L. Whitman, reports that the entire number of investigators at the laboratory during the season was 130, of whom 54 were students and 76 original investigators.

He further states that every worker at the laboratory shares the general advantage secured by the Carnegie Institution grant; that most of the occupants of the Carnegie tables were investigators of established reputation, a few of them Fellows from different universities engaged in their first original work; that it is not expected that the work undertaken will come to publication immediately, as in most cases it will necessarily extend over two or three years; that it is anticipated that the Carnegie support will not encourage hasty and fragmentary production, but will secure thorough work and permanent results.

Marine Biological Station, Naples, Italy. Grant No. 55. *For maintenance of two tables.* $1,000.

Abstract of Report.—One of the tables at this station was occupied for three months during the spring by Dr. E. B. Wilson, of Columbia University, and the other by Professor H. S. Jennings, of the University of Michigan.

The remainder of the year the tables were open to whomever the director of the laboratory might wish to assign to them.

The arrangement with the laboratory was that the tables were intended for the use of persons engaged in original biological researches, and carried with them the right to be furnished with the ordinary material and supplies of the laboratory.

Student Research Work in Washington, $10,000.

A special committee was appointed to consider the question of making provision for training in Washington students who desire to avail themselves of the various openings that may be offered to them. The Executive Committee, after full discussion, decided to place the report of the special committee on file, without action.

RESEARCH ASSISTANTS.

In pursuance of the policy approved by the Trustees at their meeting in November, 1902, the sum of $25,000 was set aside by the Executive Committee for the purpose of assisting a certain number of young investigators who have shown exceptional ability and desire to pursue special lines of inquiry, under the oversight of qualified guides, more or less authoritative, according to the circumstances of each case.

Announcement of this plan was made by a printed circular, which was published in the winter of 1902–1903, and addressed to the heads of universities, colleges, laboratories, and other scientific institutions. It reads as follows:

"It is the purpose of the Carnegie Institution of Washington, among other plans, to encourage exceptional talent by appointing a certain number of Research Assistants.

"These positions will not be those commonly known as 'Fellowships' or 'Scholarships'; nor is the object of this provision to contribute to the payment of mechanical helpers or of assistants in the work of instruction. It is rather to discover and develop, under competent scrutiny and under favorable conditions, such persons as have unusual ability. It is not intended to provide means by which a student may complete his courses of study, nor to give assistance in the preparation of dissertations for academic degrees. Work of a more advanced and special character is expected of all who receive appointment.

"The annual emolument will vary according to circumstances. As a rule, it will not exceed $1,000 per annum. No limitations are prescribed as to age, sex, nationality, graduation, or residence. Appointments will at first be made for one year, but may be continued.

"It is desirable that a person thus appointed should work under the supervision of an investigator who is known to the authorities of the Carnegie Institution to be engaged in an important field of scientific research, and in a place where there is easy access to libraries and apparatus, but there may be exceptions to this.

"Applications for appointments may be presented by the head of or by a professor in an institution of learning or by the candidate. They should be accompanied by a statement of the qualifications of the candidate, of the research work he has done, and of that which he desires to follow, and of the time for which an allowance is desired. If he has already printed or written anything of interest, a copy of this should be enclosed with the application.

"Communications upon this subject should be distinctly marked on the outside envelope, and on the inside, Research Assistant, and should be addressed to the Carnegie Institution of Washington."

In response to this announcement 127 applications were received. These were distributed according to the subjects of investigation and referred to the confidential advisers, whose written opinions were laid before the Executive Committee with accompanying papers. The persons below named were then selected:

J. H. Bair, Columbia University, New York, N. Y.	Grant No. 73.
J. W. Baird, Cornell University, Ithaca, N. Y.	Grant No. 74.
A. J. Carlson, Stanford University, California	Grant No. 75.
C. D. Child, Colgate University, Hamilton, N. Y.	Grant No. 76.
Arthur B. Coble, Lykens, Pa.	Grant No. 65.
W. W. Coblentz, Cornell University, Ithaca, N. Y.	Grant No. 77.
Lee H. Cone, University of Michigan, Ann Arbor, Mich.	Grant No. 78.
Elias Elvove, Lexington, Ky.	Grant No. 79.
Shephard I. Franz, Hanover, N. H.	Grant No. 80.
L. E. Griffin, Missouri Valley College, Marshall, Mo.	Grant No. 81.
Ellsworth Huntington, Milton, Mass.	Grant No. 82.
Herbert S. Jennings, Ann Arbor, Mich.	Grant No. 83.
George D. Louderback, Reno, Nev.	Grant No. 66.
Albert P. Morse, Wellesley, Mass.	Grant No. 84.
C. P. Neill, Catholic University, Washington, D. C.	Grant No. 85.
Hideyo Noguchi, University of Pennsylvania, Philadelphia	Grant No. 29.
James B. Overton, Jacksonville, Ill.	Grant No. 86.
H. F. Perkins, University of Vermont, Burlington, Vt.	Grant No. 87.
H. N. Russell, Kings College, Cambridge, England	Grant No. 88.
George W. Scott, University of Pennsylvania, Philadelphia	Grant No. 60.
R. M. Strong, Haverford, Pa.	Grant No. 89.
H. G. Timberlake, University of Wisconsin, Madison	Grant No. —.
J. B. Whitehead, Jr., Johns Hopkins University, Baltimore	Grant No. 59.
E. J. Wilczynski, Berkeley, Cal.	Grant No. 58.
F. S. Wrinch, Princeton, N. J.	Grant No. 91.

One of the persons thus selected, Mr. H. G. Timberlake, died in July, 1903, and one of them, Mr. C. D. Child, did not accept the appointment on account of a change in his plans. From all the others satisfactory reports of progress have been received, which again have been referred to specialists for their scrutiny and comment.

It is the purpose of the Executive Committee, upon the final examination of the reports of the work of these Research Assistants, to select those whose work justifies the opinion that their capacity warrants further grants by the Institution to enable them to proceed with their investigations, and to continue the grants to the persons so selected and discontinue them as to the remainder of the list.

The specific subjects to which these twenty-five investigators proposed to direct their attention were distributed among the following

branches of science: Astronomy, 1; Botany, 2; Chemistry, 2; Economics, 1; Geology, 2; History, 1; Mathematics, 2; Physics, 3; Physiology, 2; Psychology, 3; Zoölogy, 6.

The geographical distribution of these students cannot be very accurately stated, as their early homes are not known to the Carnegie Institution; but indications may be derived from a list of the colleges in which the preliminary academic training was received:

Augustana College	Carlson.
Beloit College	Huntington.
California, University of	Louderback, Wilczynski.
Columbia University	Franz.
Fukushima, Japan, Provincial High School	Noguchi.
Georgetown University	Neill.
Hamline College	Griffin.
Johns Hopkins University	Whitehead.
Kentucky State College	Elvove.
Lake Forest College	Timberlake.
Michigan, University of	Bair, Jennings, Overton.
Oberlin College	Strong.
Pennsylvania College	Coble.
Pomona College	Cone.
Princeton University	Russell.
Stanford University	Scott.
Toronto, University of	Baird, Wrinch.
Vermont, University of	Perkins.

It is also interesting to mention the places where their post-graduate studies were pursued:

Augustana College	Carlson.
California, University of	Carlson, Louderback, Wilczynski.
Cambridge, University of (England)	Child, Russell.
Chicago, University of	Overton, Scott.
Columbia University	Bair, Franz, Scott.
Cornell University	Baird, Child, Coblentz, Scott.
Hamline College	Griffin.
Harvard University	Huntington, Jennings, Strong.
Johns Hopkins University	Coble, Griffin, Neill, Perkins, Whitehead.
Kentucky State College	Elvove.
Leipzig, University of (Germany)	Franz.
Michigan, University of	Bair, Cone, Timberlake.
Pennsylvania, University of	Noguchi, Scott.
Princeton University	Russell.
Stanford University	Carlson.
Wellesley College	Morse.
Wisconsin, University of	Baird, Timberlake.
Wurtzburg University	Wrinch.

PUBLICATIONS AUTHORIZED.

The publication of eleven scientific papers has been authorized.

1. The collected mathematical works of the astronomer George William Hill. It is estimated that these works will make four quarto volumes. About half of volume I is printed.

2. Desert Botanical Laboratory of the Carnegie Institution, by F. V. Coville and D. T. MacDougal. This is an octavo containing 58 pages, 29 plates, and 4 text figures. Published.

3. New method for determining compressibility, by T. W. Richards and W. N. Stull. This is an octavo of 45 pages and 5 text figures. Published.

4. Waterlilies—a monograph of the genus *Nymphæa*, by H. S. Conard. This is to be a quarto containing 28 plates (12 being colored) and about 80 text figures. The text figures are made, and contracts have been awarded for plates and text.

5. Fecundation in plants, by D. M. Mottier. Manuscript received, and the drawings for text figures, about 300, have been made.

6. On the behavior of lower organisms, by H. S. Jennings. Manuscript received and accepted for publication.

7. The coral Siderastrea, by J. E. Duerden. Manuscript received and accepted for publication.

8. Catalogue of double stars, by S. W. Burnham. Manuscript ready for the press.

9. Chimera—a memoir on the embryology of primitive fishes, by Bashford Dean. Manuscript not received.

10. Bibliographic index of North American fungi, by W. G. Farlow. Will make five octavo volumes.

11. Results of investigations of poison of serpents, by Drs. Simon Flexner and Hideyo Noguchi. Manuscript not received.

RECOMMENDATIONS OF EXECUTIVE COMMITTEE.

The Executive Committee submitted a series of recommendations for the work of the fiscal year 1903–1904, which were considered and acted upon by the Board of Trustees.

The results of this action will be reported to the Trustees at the meeting in December, 1904, and be printed in the Year Book.

Applications for Grants.

The Committee has declined to make any grants in medicine or for preparing systematic treatises or essays in logic and philosophy.

All applications, from the beginning of the Institution to October 31, 1903, are summarized in the following table:

List of Applications Received from Beginning to November, 1903.

Subject.	Applications.			Amount asked for.
	Not stating amount desired.	Stating amount desired.	Total.	
Agriculture	3	1	4	$5,000
Anthropology	26	18	44	90,083
Archeology	11	5	16	17,700
Art	10	....	10	....
Astronomy	21	37	58	567,750
Bibliography	15	12	27	82,250
Biology	14	1	15	100,000
Botany	28	32	60	138,300
Chemistry	37	52	89	90,500
Economics	38	8	46	72,500
Education	20	1	21	500
Engineering	20	5	25	24,040
Exploration	2	3	5	110,000
Fellowship	39	2	41	1,700
Foreign applications	7	8	15	17,000
Geography	1	2	3	1,500
Geophysics	3	9	12	33,250
Geology	21	16	37	145,800
History	30	9	39	101,400
Inventions	21	2	23	2,100
Literature	10	....	10	....
Mathematics	11	9	20	13,525
Medicine	35	11	46	16,325
Meteorology	2	6	8	32,750
Miscellaneous	25	7	32	68,200
Paleontology	5	5	10	11,900
Philology	12	1	13	750
Psychology	22	15	37	77,600
Physics	32	26	58	37,350
Physiology	23	20	43	30,975
Publication	37	18	55	90,250
Religion	9	2	11	37,000
Zoölogy	46	63	109	182,400
Total	636	406	1,042	$2,200,398

Grants Recommended by Advisory Committees.

In addition, the Advisory Committees have submitted a number of recommendations not included in the foregoing table. These are printed on pages xxxiv–xxxv of the confidential report to the Trustees, issued November 11, 1902, and that for the southern and solar observatories in the present report:

Physics, per annum	$250,000
Geophysics, per annum	150,000
Psychology, per annum	45,000
Physiology, per annum	50,000
Southern Observatory, twelve years ($820,000), first year	80,000
Solar Observatory, twelve to fourteen years ($1,280,000), first year	150,000
History, per annum	17,500
Botany, per annum	24,000
Exploration, per annum	120,000
Geology, three years, per annum	25,000
Total	$911,500
Adding this to the total amount in above summary	2,200,398
Gives a total of	$3,111,898

The above total would have been still larger if all the grants had been made as requested. Frequently grants are requested for one year which, if made, would involve a number of subsequent grants before the completion of the work.

This is not intended as a close analysis of the amount of money desired. It merely shows the impossibility of making the present income of the Carnegie Institution provide for more than a small part of the grants requested.

Substantially all these applications have been carefully examined and considered. Many of the more important are explained in the first Year Book. All are set forth and explained in the papers on file and ready to be produced for the consideration of the Trustees.

Most of these applications have been considered unfavorably by the Committee as being for less desirable purposes for expenditure from the income of the trust than those for which grants have been made. Some, however, have seemed only less important than the matters favorably reported upon, and these should, the Committee thinks, be regarded as subjects of future consideration whenever available funds shall permit.

MEMORIALS.

Abram S. Hewitt.

Abram Stevens Hewitt, one of the confidential advisers of Mr. Carnegie respecting the foundation of this Institution, and by election the original Chairman of the Board, died at his home, in New York, January 18, 1903, aged 81 years. By his death the Trustees have met an irreparable loss. The Cooper Union, with which he was intimately associated during all its history, has published a summary of his life so excellent in all respects that the paragraphs of general interest will be here reproduced.

Abram Stevens Hewitt was born in Haverstraw, N. Y., July 31, 1822. His father, John Hewitt, an English mechanic, came to this country in 1790, to assist in erecting the first steam engine in America, and remained to share the fortunes of the young Republic. His mother belonged to a Huguenot family (the Garnier, now the well known Gurnee family) resident in the State of New York.

At the time of his birth, his father, after acquiring a considerable fortune as a cabinet maker and merchant in New York City, and losing all by a disastrous conflagration, had become a farmer upon the Garnier estate; and in a log-cabin on this tract he was born.

Working upon the farm in the summer and attending a New York public school in the winter, he won at the end of his common-school course, as the result of a severe competitive examination, one of the two free scholarships then offered annually by Columbia College, and was thus enabled to go through a college course, supporting himself by extra labor as a private tutor, so that he could afterwards say with just pride, "Not one dollar of burden did my education impose upon my parents, who anxious as they might be to give me an education, were too poor to do so."

In college, as in school, he held from first to last the position of head of his class, and, after graduation, was for some time assistant professor in mathematics. During this period he began the study of law; and in 1845 he was admitted to the bar, after an examination of exceptional severity, which the majority of the candidates failed to pass.

Meanwhile, however, he had made a brief trip to Europe, together with his friend Edward Cooper, and, shipwrecked on the return voyage, had been, with his companion, rescued after drifting for a day in a frail boat upon the Atlantic. His subsequent statement as to the impression produced by this experience is given in his own words, because it sounds the keynote of his life.

"I landed at New York in mid-winter, in a borrowed suit of sailor's clothing, and I had three silver dollars in my pocket, my entire worldly wealth.

"I was then twenty-two years old; and that accident was the turning-point of my life. It taught me, for the first time, that I could stand in the face of death without fear and without flinching. It taught me another thing—that my life, which had been thus miraculously rescued, belonged not to me; and from that hour I gave it to the work which from that time has been in my thoughts—the welfare of my fellow-citizens."

His companionship in travel and peril with Edward Cooper led to relations with Peter Cooper, in consequence of which he abandoned his intention to practice law, and formed with his friend the firm of Cooper & Hewitt, which assumed the iron branch of Peter Cooper's business, and the long, enterprising, and honorable career of which is part of the commercial history and progress of the United States.

The key to Mr. Hewitt's life is given by his words above quoted. He devoted himself to "the welfare of his *fellow-citizens.*" This is not to be construed as limiting his philanthropic sympathy to American citizens alone. Many generous acts prove that he drew no such rigid line. But, as illustrated by his life, it does show plainly that his cherished aim was patriotic as well as philanthropic. With Peter Cooper, he believed in the Republic, and in its free institutions as furnishing the necessary and sufficient atmosphere for individual well-being and progress. And he believed that knowledge, or rather the opportunity to acquire knowledge, was the one thing that could be bestowed gratuitously by the State without pauperizing the recipient. That being given, the maintenance of freedom, justice, and order would complete all that government could wisely undertake from the standpoint of internal administration.

The period of his childhood was the first age of our national history—an age of ardent patriotism and undaunted enterprise and adventure. And the inspiration of this period unquestionably continued to be with him a motive power. But his manhood was cast in a new and different age—that of the material conquest of a continent, the defense of national unity, and the development of industry and commerce. The questions thus encountered called for not only stanch patriotism, but also a knowledge of law and practical business; and in these respects Abram S. Hewitt was thoroughly equipped for illustrious service.

During the war of 1861–5, though always politically a Democrat, he gave the government a hearty support. In fact, the iron works of Cooper & Hewitt were for four years largely given up to the manufacture of munitions of war, without profit to the firm, but (as the War Department has repeatedly acknowledged) to the great and in some instances decisive advantage of the Union cause.

In 1867 Mr. Hewitt was one of the U. S. Commissioners to the Paris Exposition, and his report on the manufacture of iron and steel, as illustrated at that exposition, produced a profound impression at home and (through translations in several languages) abroad. Years before, his firm had made experiments with the Bessemer process; and after 1867, it undertook the introduction into America of the "open-hearth" process. Still later, it was concerned in the adoption of the "basic-lining" principle, applicable to both these methods; and thus it may fairly be said to have had a vital connection with the methods which now cover the manufacture of nearly all the steel (except the so-called crucible-steel) of this country and the world. For his services in connection with these great improvements, the Iron and Steel Institute awarded to Mr. Hewitt, in 1890, the "Bessemer Gold Medal." Meanwhile, the iron-works of Cooper & Hewitt in various localities exhibited the results of the latest scientific practice, and stood, at times, at the head of American practice—a position which no single concern can long continue to hold.

In 1876, and again in 1890, Mr. Hewitt was elected President of the American Institute of Mining Engineers; and his presidential addresses during both terms

attracted wide attention and have become classics in the literature of the subjects with which they deal.

His important and influential Congressional career began in 1874, when he was elected to the House of Representatives. Reëlected in 1876, he declined in 1878 to be a candidate; but in 1880 he was again returned, and held his seat by successive reëlections until, in 1886, he was chosen Mayor of New York.

His work in Congress was more profoundly important, perhaps, than has ever been recognized. While he often differed with the leaders of his party, he was always relied upon by them, and even by his party opponents, for wise advice and intelligent information. In the reform of the consular service, the resumption and maintenance of specie payments, the defeat of the "free" coinage of silver, the establishment of the National Geological Survey, and the peaceful solution of the Presidential electoral contest of 1876, his attitude and arguments may be said to have been decisive factors. To the various tariff debates, he contributed a business knowledge, sometimes distasteful, but always useful, to theoretic partisans. The successive Morrison, Mills, McKinley, and Dingley tariff bills embodied, apart from their schemes of duties, administrative details which he had furnished. He secured also the adoption of the extensive plan for the improvement of New York Harbor, which since has been steadily prosecuted.

In 1886 he was elected Mayor of New York City. To the duties of that office he devoted with intense assiduity the powers which had been developed and matured by a lifetime of varied experience; and his administration, though hampered by local partisan conditions, left behind it many beneficent effects and many suggestions which have since borne fruit. Of these, one of the most permanently important is the plan of municipal rapid transit which he devised, and which, though at first ignored by the Board of Aldermen and smothered in the legislature at Albany, was at last forced to adoption by the public sentiment, and is now in process of execution. Its essential feature is that the work, constructed with the money, and actually the property, of the city, shall be leased to a responsible corporation at a rental covering both the interest on its cost and a sinking fund which will repay in fifty years the principal; so that, at the end of that period, the whole rapid-transit system will be the property of the city, free of all cost, even of interest *ad interim*, and of all obligations to any private or corporate interest. A fairer or more ingenious method of dealing with a great public franchise, without imposing burdens upon the present generation for the benefit of the next, it would be difficult to devise.

In April, 1900, the Chamber of Commerce of the State of New York elected Mr. Hewitt to honorary membership, in recognition of "his long and valuable services to the city, State, and nation, and with special regard to his initiation in this body, of the rapid-transit plan, under which the contract was awarded, and the work is now proceeding." At the same meeting, a gold medal was ordered to be prepared and presented to Mr. Hewitt, for his services in the cause of rapid-transit under municipal ownership.

For more than forty years from the beginning of the Cooper Union until his death he was one of the trustees and the Secretary of the Board. During the whole period he was practically the General Superintendent of the Institution, and with great capacity and untiring devotion supervised its work and affairs and devised and carried into effect plans for the extension of its usefulness. His

personal contributions in money have been over two hundred thousand dollars, and no doubt it has been largely due to his personality and to the work he has done for the Cooper Union that generous donors not related to Mr. Cooper have made it the recipient of their munificent contributions to its endowment fund.

William Earl Dodge.

William E. Dodge, of New York, was the first person elected by the Trustees of the Carnegie Institution to membership in the Board. Their estimate of his character has been given upon a preceding page, and now a brief summary of the events of his life will be added.

Our colleague bore the full name of his father, William E. Dodge, once a member of Congress, eminent for his virtues as a merchant, a philanthropist, and a public-spirited citizen. The son was born in New York, February 15, 1832, and throughout his long life was connected, in one capacity or another, with most of the commercial, religious, and beneficent institutions of the metropolis. In many of them he had an hereditary interest. For fifty-three years he was connected with the house of Phelps, Dodge, and Co., of which at his death he was the senior member. As his years advanced, his influence increased, and his counsel was sought in the promotion of many important enterprises. Originally engaged in the importation of metals, he subsequently took part in various mining and manufacturing industries and in railroad transportation. He became a Trustee of the Metropolitan Museum of Natural History, and of the Botanical and Zoölogical Gardens of New York. The Columbia School of Mines honors him among its earliest friends. He gave liberally to many colleges, and among them he was particularly interested in the Teachers' College of New York. Of the Young Men's Christian Association in New York he was one of the earliest members, and of the Evangelical Alliance a lifelong supporter. In the promotion of Southern education he was a wise counselor, and the advocacy of international arbitration lay very near his heart. In the lapse of time his services in the United States Sanitary Commission may have passed from remembrance, but they were important, and recently his influence in the monetary conference at Indianapolis was most useful. His good deeds are well enumerated in the following words by Dr. L. T. Chamberlain, who knew him well:

> His executive force was in keeping with his rare perception. In passing from principle to practice, he lost no whit of his preëminence. He knew men. He understood means and measures. Though not of stalwart physique, his capacity for toil was prodigious. Neither did difficulties appal him, nor delays

discourage him. When he had resolved on the desired end, no course was too laborious, and scarce any process too costly, for his self-denying adoption. Witness his organizing work in the Sanitary Commission of the Civil War; his guidance of the great plan of Young Men's Christian Associations; his part in the development of the Students' Volunteer Movement; his furtherance of the cause of Arbitration between his country and Great Britain; his promoting of the Federation of Churches and Christian Organizations in this city; his service in connection with the Peabody and Slater Funds; his quiet, effective advocacy of "sound money"; his labors as Chairman of the Committee of One Hundred on India Famine Relief; his unmeasured and priceless devotion to the Metropolitan Museum of Art, the Museum of Natural History, and the Botanical Garden; his practical interest in all matters of civic and social betterment; his influential place in nearly all the great movements of the Chamber of Commerce, in his day; his carrying forward of the notable Conferences held by his Alliance; to say nothing of his successful control of his own varied and important business undertakings.

And the same writer adds this appreciation:

Though not scholastic after the manner of the schools, he was truly widely cultured. Notwithstanding his unremitting preoccupations as a man of affairs, his reading was extensive, and what he read became his own through comprehension and remembrance. Those who knew him best most wondered at his real acquaintance with the noblest books. Nor was his acquaintance with noble men and women less ample. Here, and abroad, and in every walk in life, he had friends whom it was well worth while to know. His home was hospitable to the wise and good from far and near. With the worthiest he had exalted friendship. He was a good writer. He was a good critic of writing. He was an accomplished speaker.

Though the inheritor of wealth, and himself the creator of added wealth, he was his own master. His abundant possessions found him, and left him, free. His soul lost no freshness, no grace, by reason of wealth's vain-glory. He felt the obligation and the honor of Christian stewardship. He gave gifts quietly, joyfully, like a prince. His benefactions were constant and large. The law of the tithe was not enough for him. He measured his privilege by the greatness of the need and the extent of his resources. With his gifts he gave himself.

After a long illness, the death of Mr. Dodge occurred at his summer home in Bar Harbor, Maine, August 9, 1903.

MARCUS BAKER.

Marcus Baker, the Assistant Secretary of the Carnegie Institution, performed his last work in editing the present volume. He had been in failing health for some months, and the end came while the book was on the press. He was a scholarly man, of broad culture, and talented in many fields; he was a conscientious, painstaking, accurate man, doing thoroughly and well that which he undertook;

and he was an optimistic and affable man, who delighted to be of service to others. His activities were so varied and his responsibilities so numerous that the Carnegie Institution is but one of several organizations to suffer by his loss; yet the Institution will miss him most, for he had assisted in its organization and knew all the details connected with its affairs. He was a man of science, occupied chiefly with geographic and bibliographic researches, but a contributor also to history, and a lifelong student of mathematics. Though possessing no capital and not engaged in business in the ordinary sense, he yet held several positions of trust in business organizations. He had also completed a law course, and was competent for admission to the bar. An outline of his life is given below.

He was born at Kalamazoo, Michigan, on the 23d of September, 1849, the earlier part of his youth being spent on a farm, and the later in the city of Kalamazoo. His education began in the common schools of Michigan. Two years were spent in Kalamazoo College and two in the University of Michigan, from which he was graduated in 1870. The degree of LL. B. was received from Columbian University in 1896.

In the summer after graduation he assisted Professor Watson, of Ann Arbor, in computations for the *Nautical Almanac*. Then for a year he held the chair of mathematics in Albion College, and for two years was instructor in mathematics in the University of Michigan. He then availed himself of an opportunity to enter the United States Coast Survey, and was a member of that corps for thirteen years. He assisted Dr. W. H. Dall in surveys of the coast of Alaska, having for his special function the astronomical determination of latitude and longitude. Afterward, at the offices of the Survey in San Francisco and Washington, he aided in the preparation of the *Coast Pilot of Alaska* and of a bibliography of the geography of Alaska. In later years he compiled for the Geological Survey a dictionary of Alaskan names. In 1882 he was sent to Los Angeles, California, by the Coast Survey to install and conduct a primary magnetic station or observatory, and he was afterwards assigned to an investigation of the tides and currents of New York harbor and their relation to the coastal bar and other shoals.

In 1886 he resigned to accept a position in the United States Geological Survey, and he was connected with that organization until the founding of the Carnegie Institution. For a number of years he had charge of the northeastern topographic division, supervising the mapping of Massachusetts, Rhode Island, Connecticut, and a

part of Pennsylvania. He was afterward editor of topographic maps. While these were his principal routine duties, they occupied only a portion of his time. He was from time to time entrusted with various special researches, usually of a literary or bibliographic character, for the purpose of aiding the director in the preparation of special reports and other documents. He represented the Geological Survey on the Board on Geographic Names, and for more than ten years was the secretary of that board, having charge of its files and collating the recorded usage of most of the names submitted to the board for decision. He was also the editor of its bulletins.

When a commission was appointed by our government to investigate the matter of the Venezuelan boundary, Mr. Baker was employed as geographic expert, taking leave of absence from the Survey for that purpose. He prepared a compendious report, including an exhaustive bibliography of the maps bearing on the boundary dispute. Afterward, when arrangement was made for arbitration, he was employed by the counsel for Venezuela, and spent two years on the preparation of the case.

Mr. Baker was member of a number of scientific societies, from which he accepted duties that occupied much of his leisure. For several years he was secretary of the Philosophical Society of Washington and editor of its *Bulletin*; and afterward he was its president. He was an officer of the Geographic Society from its organization, and also of the Historical Society. When the scientific societies of Washington became affiliated through the constitution of a joint commission, he was chosen its secretary, and in that capacity began the preparation of the joint directory of scientific societies, which he continued from year to year. In the same connection he was made secretary of the local committee for the entertainment of the American Association for the Advancement of Science in 1891. When the joint commission was succeeded by the Washington Academy of Sciences, in 1898, Mr. Baker was chosen not only to the Academy but to its board of managers, and he afterward became the editor of its *Proceedings*.

Mr. Baker in 1874 married Sarah Eldred, who died in 1897. In 1899 he married Marion Una Strong, who, with two children, survives him. His death occurred December 12, 1903.

ACCOMPANYING PAPERS

CONTENTS OF ACCOMPANYING PAPERS.

REPORT OF COMMITTEE ON SOUTHERN AND SOLAR OBSERVATORIES

CONTENTS

The undersigned, members of a Committee appointed to consider certain large projects in astronomy, have given close attention to that duty and herewith present the conclusions reached. Other matters still pending or requests from the Trustees may necessitate further communications which we shall take pleasure in presenting as occasion may arise.

I. General Recommendations.

We strongly urge the adoption of each of these projects submitted to us for examination, so far as this can be done consistently with other obligations which the Carnegie Institution may feel bound to assume. Our recommendations refer to definite programs of scientific work to be accomplished and not to the establishment of permanent institutions. Should these programs be carried to a successful issue, we are aware that in the meantime other demands for astronomical work quite as important and not less pressing may arise. The question whether the Carnegie Institution shall consider it advisable to take them up is one which we will not attempt to discuss at the present time.

For the present we urge the establishment of an observing station in the southern hemisphere for the accomplishment of certain definite works of observation, so arranged that their completion may be anticipated within a period of ten or twelve years from the time of beginning.

We also urge the establishment of an observing station for solar investigation in exceptionally favorable conditions of atmosphere, to be kept up during one full sun spot period at least (eleven years), or, preferably, through the maximum which may be expected to occur about 1916. In connection with this station (or with both stations) we recommend the construction and maintenance of a powerful reflecting telescope, as large as it is thought prudent to undertake, for use in astrophysical investigations upon the stars.

In appropriate sections of this report and in the appendices the several projects are discussed in some detail. We are indebted to our scientific colleagues for important advice and suggestions upon the matter herein treated, for which our best thanks are due.

These projects involve a large expenditure of money and scientific effort. Their importance demands that the reasons justifying an expenditure so great should be stated somewhat in detail, and that the character and importance of the scientific works for which provision is desired should be indicated.

Before proceeding to these details, however, it may be well to take up some of the general considerations that have a bearing on these projects.

Under its policy, as we understand it, the Carnegie Institution would not be inclined to establish an astronomical observatory for the general promotion of astronomy without more definitely expressed object. In lieu of special instructions the Committee looks for guidance to the declared policy of the Trustees as found in the Year Book for 1902 (pp. xxxvi–xxxvii). From this it appears that:

> "The Institution does not propose to undertake—
> (*a*) To do anything that is being well done by other agencies.
> (*c*) To enter the field of existing organizations that are properly equipped or are likely to be so equipped."

Aside from the support which it is the policy and practice of the Carnegie Institution to extend to investigations to be carried on through existing agencies, it desires

> "To promote original research by systematically sustaining—
> (*a*) Projects of broad scope that may lead to the discovery and utilization of new forces for the benefit of man.
> (*b*) Projects of minor scope that may fill gaps in knowledge of particular things or restricted fields of research."

These declarations seem to mean that the Instituution desires especially to assist in repairing notable deficiencies in knowledge. There is a kind of balance in the progress of various branches of research which it is desirable to maintain, so that results in one line of inquiry shall not remain too long unused in the archives of science while another line of investigation is bringing what is necessary for their proper interpretation. A virtual deficiency in knowledge may also be created through the neglect of opportunity to undertake some promising line of investigation, caused either by the forbidding cost of preparation or by some special inconvenience attached to its prosecution.

It will not be difficult to show, under this interpretation of the policy of the Trustees, that the projects considered here are singularly adapted to the support of the Institution.

Drawbacks to be considered.—It is proper, however, that we should call attention to one of the drawbacks which might result from the establishment of either of these observatories upon a temporary basis. The withdrawal from their present relations of a large number of competent astronomers for temporary duty with these observatories must be productive of some degree of disorganization in those institutions from which they may be recruited. The return of these men to more permanent relations may also be attended with more or less uncertainty, in view of which it is obvious that the compensation of the astronomers required for temporary duty must be decidedly larger than would be necessary under other conditions.

Another disadvantage of a temporary as compared with a permanent organization is the large proportion which the expense of installation bears to the total expenditure involved, together with the fact that the termination of the programs proposed would leave this expensive equipment idle on the hands of the Institution, unless some further provision should be made for its useful employment. This further use, however, could be effected in either of two ways:

(1) By provision for further employment on the part of the Institution.

(2) By donation or sale of the instruments to existing institutions which are in a position to make good use of them.

Relation to existing institutions.—The Committee desires also to record, in the most explicit manner, its opinion that nothing which may be determined in relation to large projects in science should be allowed to impair the total amount of support which the Institution

now extends in aid of astronomical research at existing institutions, so long as the results obtained shall show that such aid is efficiently expended. By the encouragement of several active centers of astronomical activity the amount of valuable astronomical product is stimulated out of all proportion to the means expended, and the future development of a number of able astronomers is made more certain than would be the case in a policy of greater centralization.

PROPOSED OBSERVING STATION IN THE SOUTHERN HEMISPHERE.

The project for a Southern Observatory was advocated in the general report of the Advisory Committee on Astronomy for 1902. Among other things, the committee says:

> "The third point which has specially impressed itself upon our attention is the great deficiency of observatories in the southern hemisphere. * * * Since more than one quarter of the entire celestial sphere is efficiently reached only from the southern hemisphere, it is obvious that there is now very great disparity of astronomical resources to the disadvantage of the southern hemisphere. * * * We regard this question to be exceeded in importance only by the urgent need of provision for current work to which we have already alluded." (*Year Book* for 1902, pp. 89–90.)

The matter was further discussed in Appendix A and elsewhere in the reports of the committee.

We advocate the establishment of another active astronomical station in the southern hemisphere because there is needed in certain special lines a much greater output of astronomical observations, which can be obtained only by means of an observatory in some part of the southern hemisphere. This need veritably exists, as we shall attempt to show. It has grown out of the present progress of astronomy. The satisfaction of this need seems possible only through the aid of the Carnegie Institution.

What, then, is this need? We attempt to answer this question more fully in the section of this report devoted to special consideration of the proposed Southern Observatory; but we may be permitted to touch upon it briefly in another way here.

For three centuries astronomy has been developing by a rapidly increasing ratio of progression in attack upon the most accessible sources of knowledge. Until after the middle of the nineteenth century it was almost wholly absorbed in

(*a*) The study of the shape and dimensions of the earth and other terrestrial problems;

(*b*) An investigation upon the motions of the bodies associated together in the solar system.

The problems encountered in these two lines of research are of singular fascination, and they are capable of development in essential points upon the basis of observations that we now regard as comparatively crude. The element of time was favorable. Observations extending over relatively few years are sufficient to develop the facts of planetary astronomy in their broad outlines. It was natural that these subjects should have received the first attention of astronomers. The number of separate objects concerned was small, so that the work of observation did not bear a relatively large proportion to that of mathematical development of them.

But the time came, during the last third of the nineteenth century, when astronomers began to feel satiated with their conquests in the terrestrial and planetary fields. Facts and observations relative to the astronomy of the stars had been slowly accumulating. Plans were made and carried out to increase the extent and value of these observations. The *non possumus* of the more conservative astronomers of the old school fell upon less and less willing ears. The influence of the ideas which led to the establishment of the great Pulkova Observatory, sixty years ago, began to be more and more felt. On all sides it is perceived that the *sidereal problem* is to be the astronomical problem of the twentieth century, as the planetary problem was the characteristic problem of the eighteenth century.

Thus a great movement has been inaugurated for the accumulation of facts and observations in sidereal astronomy. The scope and meaning of the movement may be learned from a brief review of a part of the work accomplished or in progress :

(1) We are witnessing now the greatest activity ever known in the history of astronomy for the accurate, systematic, and extensive measurement of the positions of the principal fixed stars at successive epochs.

(2) We have seen the plan of the *Astronomische Gesellschaft*, for the accurate observation of all stars down to the ninth magnitude in the northern heavens, brought to the point where its success is fully assured. This is by far the most extensive work in astronomy ever attempted, previous to the project for the Astrographic Chart next to be mentioned.

(3) We are now watching the successful prosecution of observations for the Astrographic Chart—the attempt to measure from photographs the accurate positions of all stars down to the eleventh,

and to chart all down to the fourteenth magnitude. This gigantic project has been taken up with a degree of faith and a force of determination which proves the widespread interest that is now felt in sidereal problems.

(4) There is remarkable growth in the number of successful attempts to measure the motions of stars in the line of sight. Twenty years ago it seemed almost impossible to make these measurements with sufficient accuracy; now, at a few observatories it has become almost a matter of routine; and seven or eight of the largest telescopes in the world are dovoted in part to this work.

(5) Until comparatively recent years the measurement of the distances of stars was taken up here and there; but rarely did a single astronomer attempt to make this measurement for more than two or three stars. Now this work is carried on successfully for series of stars by observers who are devoting many years to it.

All of these works, and many others not here enumerated, have been undertaken for the purpose of throwing light upon the astronomy of the stars, in and for itself.

But activity in these and similar lines is almost wholly confined to the northern heavens. The resources required for extending these researches over the southern sky are wanting, except for the Astrographic Chart. Even for that it is a question whether the present or prospective resources of the southern hemisphere will prove sufficient.

Certainly there is no one subject in physical science that seems better entitled to command some part of the interest of every intelligent man than that which relates to the structure and mechanism of the vast aggregation of stars and nebulæ which challenge the curiosity of all beholders. The scale upon which the visible universe is constructed, and the inconceivably rapid as well as perplexing motions which prevail among the bodies that it contains, propose to our minds problems which have a high degree of interest, both physical and philosophical. The nature of these problems we will touch upon more fully in a subsequent section of this report; and we shall there endeavor to show that there are problems which justly concern astronomers of the present generation. There are some secrets of Nature which may forever remain concealed from the eye of research. We do not know. But problems which concern geometrical relations and motions can be solved when time and opportunity are propitious. From what has been already learned about the structure and mechanism of the universe of stars, it is easy to see that very much more must be very nearly in sight.

Now, precisely the greatest obstacle to a clear view of the stellar problem is the comparative lack of information about the stars in the far southern sky that are invisible to northern observers. When a sufficient number of accurately observed facts concerning these shall have been obtained, research upon the sidereal problem will have received a strong impulse forward. If for any reasons it is desirable to know the accurate distance between two points, no expenditure of labor and skill upon the measurement of three quarters of that distance is of full effect if our knowledge of the remaining fourth depends upon a rough reconnoissance. The observational situation in the southern hemisphere is fairly illustrated by means of this comparison, which also illustrates how the execution of the proposed observations in the southern hemisphere would increase the value of what has been already accomplished by northern observers.

In the directions where we were most likely to obtain trustworthy information as to the probabilities we have made careful inquiry to ascertain whether there is any present likelihood of increase in astronomical activity in the southern hemisphere through existing agencies, and we have not only failed to learn of any such likelihood, but, as will be shown, it has become clear that the present and prospective resources of the southern hemisphere are scarcely adequate to the fulfilment of existing obligations, for which we had supposed that adequate provision had already been made.

The works of observation that we propose could probably be brought to a successful conclusion within ten or twelve years from the time of beginning. While we are confident that the necessity for maintaining the proposed observing station for a still longer period after the expiration of its first mission would be as keenly felt then as now, that will be a question for the future to decide in the light of experience. Funds of the Institution need not be tied up for this purpose in perpetuity.

Thus we have in the proposed Southern Observatory two important requisites that would seem to commend it to the favorable attention of the Carnegie Institution :

(*a*) A project of broad scope, embracing also features of minor scope, that may lead to important discoveries and that will certainly "fill gaps in knowledge of particular things."

(*b*) It does not "enter the field of existing organizations," and does not propose "to do anything which is being well done by other agencies."

In the section of this report which deals with the proposed Southern Observatory various definite works are enumerated with particular care, and reasons are given in each case why it is important that they should be executed at this time. An effort has been made to indicate the bearing of these works upon the current of astronomical research, proving the manifold benefits which are certain to accrue to a numerous class of investigations. Should this scheme be carried out to a successful conclusion, it will hereafter be found that the results will become interwoven with the progress of knowledge. The points of contact and of essential support will become numerous beyond the power of present estimation.

What are the chances of success in carrying out the project should it be undertaken? This is certainly a pertinent question. We shall not attempt any over confident prediction in this direction. Much depends in a matter of this kind on the manner in which the enterprise is conducted, and also upon the point whether the means are secure and proportioned to the end; but we are able to say with confidence that there would be few obstacles in the way of success. The importance of the various works is incontestible; this should inspire those who are to execute them. The observers need not be troubled by the least suspicion of the futility of what they are doing. The success of the work does not depend upon the success of hypothetical experiments. It is not proposed to do anything by a process so novel that the guidance of experience in any material degree will be wanting. Even the probable duration of the works can be quite accurately gauged through the teachings of abundant experience. From the outset, in all really essential features, the observers will know what they ought to do and how they ought to do it.

The questions of organization and choice of site for the proposed observatory in the southern hemisphere present some problems which call for careful thought. The project admits of various scales of expenditure, but it is earnestly to be hoped that the scale adopted by the Institution would be sufficient to cover all of the works recommended.

Proposed Observing Station for Solar Investigation.

Among the projects commended by the Advisory Committee on Astronomy in 1902 is that for the establishment of an observing station for solar research at an elevated point where the atmospheric conditions should be of exceptional excellence. Closely associated

with this project and equally commended to favorable attention was another project for the construction and maintenance of a large reflecting telescope to be used in stellar researches.

In the domain of astrophysics there appears to be no direction in which there is a more hopeful prospect of a marked advance in knowledge to follow a large investigation wisely planned.

It is not surprising that the nature of the sun should have been an object of speculation from the earliest times. In the estimation of mankind generally the sun seems to be obviously the most important of the celestial bodies. From the astronomer's point of view, however, the sun has another interest which is not less intense and not less impressive.

The sun is a star. It is the only star near enough to us to permit of detailed study into its physical constitution. The next nearest star of whose existence we are now aware is nearly three hundred thousand times more distant than the sun. It should not be difficult to understand that if we wish to study the physical condition and history of stars under such difficulties of distances, it would be an immense gain if we could attain detailed knowledge as to any one of them which might happen to be near. The sun offers this opportunity.

It is already known that a great variety of successive stages of development seem to be illustrated in the analysis of the spectra which the stars present. There appears to be no link missing from the hottest and brightest to the coldest and least luminous. In the study of this development, then, it is manifest that we should gain a great advantage—the key to the position—if we could thoroughly know the physical constitution and tendencies of one in this series of stars. The sun is the one which offers this opportunity.

But astronomers have sought to take advantage of this opportunity during three centuries. For the last fifty years an immense total of effort has been expended on this research. For more than forty years the spectroscope has been in the hands of astronomers as an effective weapon of solar research. There are numerous institutions where solar research is now actively carried on. Where, then, is the field which would justify a special effort on the part of the Carnegie Institution to enter upon solar research at the present time?

The expanding record of our knowledge relating to the physical constitution of the sun is parallel with that of increase in the power of the appliances used in this research.

First, we have the improvement of the telescope. In its application to this purpose, we believe that this improvement has by no means reached its limit.

Second, we have the development of spectrum analysis—the study of light radiations in the spectrum. Rapid improvement in the apparatus which this method employs has been continuous for nearly forty years, and seems to be more strongly marked now than ever before. This is well illustrated in the recent development of the spectograph and the spectroheliograph, and in the marked advance brought about in the size and quality of diffraction gratings.

Third, there has been developed within the last quarter of a century the power of accurately investigating the distribution of heat radiations in the spectrum of celestial objects, and especially in that of the sun. An immense advance in knowledge has been recorded in this way through the accurate representation of a large portion of the solar spectrum containing rays invisible to the eye and beyond the reach of the photograph.

These facts alone might not be sufficient to fully warrant the project of combining in one large establishment the harmonious cooperation of these three methods of research developed to the highest state of science at the present day, but they should certainly be sufficient to excite interest in the question.

The present state of our knowledge of the sun is surprisingly deficient in comparison with that which would seem to be attainable by the use of the most powerful appliances. The fact that these appliances have not been brought to bear in anything like the extent demanded by needs well understood affords no very hopeful outlook in the immediate future that these needs will be met at existing institutions. What immense additions to knowledge are possible through the intelligent invention and use of powerful instruments of research in this line is well illustrated in the normal solar spectrum produced by Rowland less than twenty years ago, by methods remarkable for their simplicity, which at once superseded all the laborious efforts in that direction resulting from the use of less powerful apparatus during the preceding thirty years. We believe that similar opportunities for progress in solar research are within the reach of investigators of the present day through the provision of apparatus of which the possibilities are now generally conceded.

A distinct advantage might also result in the attempt to carry on in one institution the three related methods of investigation—tele-

scopic, either visual or photographic; spectroscopic, involving spectrum analysis in its various forms; and bolometric analysis of heat radiation. These may be likened to three senses by which solar phenomena can be apprehended and described. One of these senses may become aware of some passing and suggestive phenomenon in regard to which the evidence of the other senses will be immediately required. There would appear to be a strong likelihood that a great gain will be experienced in the study of local developments on the surface of the sun simultaneously from three points of view.

The chief obstacle to progress, however, appears to reside in atmospheric disturbances which prevent the full advantages that otherwise might be realized from the use of powerful optical appliances. There is scarcely any department of astronomical research wherein this difficuly is more acutely felt. Very naturally nearly all astronomical institutions founded by governments and universities have been located in or near capital cities or other great centers of population, where the manifest advantages and stimulus of a scientific atmosphere may be obtained. There is doubtless the desire also to maintain the visible connection between them and the power by which they were created. The observatories of Paris and Berlin are in the heart of great cities. In the case of very few existing observatories can it be said that the choice of site was influenced in any marked degree by a consideration of superior advantages in atmospheric conditions.

But superior atmospheric conditions are precisely what solar research most requires. Where these do not exist, small telescopes frequently answer almost as well as large ones. The air is in an almost continuous state of agitation, which prevents the employment of high optical power. Dust and smoke absorb a portion of the solar radiations, and these in an unequal degree. If we are to establish a solar observatory in ideal conditions, we must seek to avoid these disadvantages. While we shall not be able to accomplish this perfectly, we ought to inquire what gain may be fairly within reach.

Although the Carnegie Institution is free to locate a solar observatory anywhere in the world where this can be done to the best advantage, we have still thought it likely that the proper balance of advantages can be best secured within the United States. For some purposes a subtropical station might offer peculiar advantages for certain sections of this research. If the observatory had but one thing to do, and if the methods of observation could be formu-

lated and reduced to routine in advance, the arguments in favor of a subtropical station might be stronger than they are; but we know of no instance of the development of an important research outside the temperate zones. One of the highest obligations of the proposed observatory would be to follow up and take advantage of the indications brought out by successive programs of observations. Interpretation of results and choice of new and promising lines of attack would probably be the yearly experience of such an observatory, even though the main current of its work would consist of comparative observations maintained through a long period.

In order to secure the greatest freedom from atmospheric disturbances and from the absorption of solar radiations by the atmosphere, one might suppose that an extremely high elevation, where the observer would have below him as much of the atmosphere as possible, should be chosen. How great this elevation ought to be is involved in more than one consideration. The cost of maintenance would always increase very rapidly with increasing altitude. This becomes an important obstacle where a large observatory is in question. At elevations much above 10,000 feet extremes of temperature are likely to prevail, hurtful alike to the action of the instrument and to the observer. The rarity of the atmosphere in producing mental enfeeblement and prostration is a most serious drawback at extremely great elevations. No doubt men accustomed to sea level conditions can also live at altitudes exceeding 15,000 feet, if they have nothing in particular to do; but if they are to be called upon for prolonged and intelligent exertion, the case is different. Furthermore, it is quite unlikely that the most favorable atmospheric conditions are to be found above a certain moderate elevation.

These considerations moved us to seek an elevation of from 4,000 to 6,000 feet for the main observatory, with the idea of selecting a suitable auxiliary station at a much higher elevation for certain observations not requiring great steadiness of atmosphere, but needing a transparent atmosphere.

Our preliminary examination of climatic conditions seemed to indicate that a suitable station could be found in California. The steps which led to the provisional selection of Mount Wilson, a few miles east of Los Angeles, as the main station for the proposed solar observatory are detailed in the section of this report appropriate to that subject, and in the report of Mr. W. J. Hussey (Appendix A), who was employed to make the requisite explorations and atmos-

pheric tests. The atmospheric conditions on Mount Wilson at an altitude of 6,000 feet, as reported by Mr. Hussey, appear to be remarkably fine, and this opinion is supported by Messrs. Campbell and Hale, who took part in the final tests.

Auxiliary station for solar observations.—Among the problems of the proposed observatory would be that of determining what is the total amount of heat radiated by the sun and to what extent, if any, this amount varies—the determination of the so-called solar constant. For this purpose it would be ideal to make this measurement from a point which receives the radiation of the sun without any absorption whatever by the earth's atmosphere. As an approximation to this, one might choose a station upon the top of the loftiest mountain which can be ascended; but for reasons already stated we are not in favor of this extreme elevation.

It has been thought that some such site as Mount Whitney, about 170 miles north of Mount Wilson, might offer a suitable auxiliary station for the special observation of heat radiations in relation to the solar constant. The altitude of this mountain is nearly 15,000 feet. This is an elevation beyond which it can scarcely be supposed that continuous observations could be effectively carried on. In 1881 Professor Langley occupied this peak for a similar purpose. We think it might ultimately prove that after occupation for two or three seasons such a relation with the results obtained on Mount Wilson might be established as to render the continued maintenance of such an auxiliary station unnecessary. The principal study of the heat radiations of the sun would then be made at the permanent station on Mount Wilson, or wherever the main station might be located.

In the section of this report devoted to the proposed Solar Observatory will be found a presentation of reasons for our conviction that this institution should be established, together with a carefully prepared description of the appliances that would be required.

Advantages of a great reflector.—As connected with both the Southern and the Solar Observatory, and a consistent part of them, we think that a large reflector should be mounted and maintained for the investigation of stellar spectra. From what we have already stated, it will be seen that we regard the relation of solar physics to stellar physics the most important inducement for the establishment of the Solar Observatory, though not the only one. In order to make a comparative study effective, there would be a great advan-

tage in associating the two branches of study, solar and stellar, in one establishment. But, aside from this, we think that the site which has been provisionally selected (or a still better one if it can be found) would be most admirably suited as the location for the most powerful telescope that can be constructed. The occupancy of this site for such a purpose seems to offer a rare opportunity that ought not to be neglected.

Furthermore, it is now conceded that the construction and successful operation of a reflecting telescope, having a mirror five feet in diameter, is entirely feasible. We have the benefit of experience, both at the Lick and Yerkes observatories, in demonstrating the great advantages to certain researches in astronomy which may be derived from the use of large reflectors. We feel no hesitation in offering the prediction that researches in astrophysics would be enriched to a remarkable degree by the use of the largest reflectors that can be made, and we would strongly urge the advisability of providing a very large reflecting telescope as part of the equipment of the proposed observatories.

Policy of proposed Solar Observatory.—Plans and estimates for a Solar Observatory, and likewise for a Southern Observatory, have been prepared upon our understanding that the Carnegie Institution is not in favor of establishing permanent observatories, but that works undertaken must be in the nature of expeditions for the solution of definite problems, in limited periods of time. In some ways this policy seems to us to have advantages, especially in relation to problems for whose solutions the instruments are of standard form and for which the methods have been perfected and thoroughly tested. Examples of this are meridian circle, heliometer, and micrometric work generally. On the other hand, this policy is not a natural one for those new works which call for developments in instruments and methods. Some portions of the solar work and the application of photography to parallax problems are examples of this.

The task proposed for the Solar Observatory is only in part of a routine character. A large part of its work ought to be one of development, in relation to which it would be difficult to foresee, in all its details, the exact character of the equipment which might be best calculated to meet successive requirements of investigation as they may arise. Consequently, we are of opinion that the director of the proposed observatory should not be hampered with a rigid program of method and equipment, prescribed in advance.

Problem of Organization.

It seems almost superfluous to add that the prompt equipment and organization of two observatories such as we have recommended might be a temporary strain of some severity upon the existing resources of astronomy. The only remedy for this which we can suggest is the avoidance of undue haste. There can be no question that the success of these enterprises would depend very largely upon the ability and energy of the agents selected to carry them out. It may safely be said that no enterprise in science is so important that it cannot afford to await the appearance of scientific men of sufficient foresight to appreciate its importance and of sufficient ability to carry such an enterprise to fruition. On the other hand, we think that it would be far more logical for the Institution to determine from the consensus of astronomical opinion what is best to be done in astronomy and then to take means to get that thing done, than it would be to build and equip an observatory with instruments and staff in the hope that such staff might find something profitable to do. In the former case it is but a question of time in finding the astronomer, previous to which no expenditure is lost; in the latter the expenditure is incurred at the outset and remains to be justified. The latter course is unavoidable in the case of governments and institutions which establish permanent observatories, and on the average it produces good results; but the Carnegie Institution enjoys the rare privilege of taking the former course, which we believe to be the safest where it can be employed.

II. Proposed Observatory in the Southern Hemisphere.

In inviting the attention of the Trustees to the project for an observing station in the southern hemisphere, we are not proposing a mere generality. We propose certain definite things to be accomplished.

This undertaking essays the humbler task of producing the facts of observation by means of which future hypotheses must be tested and upon which theories must be founded, rather than the more brilliant role of attempting some great scientific generalization; yet it does not altogether lack the attractiveness which belongs to great discoveries, since it is intended to prepare a road by which discoveries can be reached. In any future division of honors it cannot fail to win its share. In fact, it is the more sure of this because theories often fail and are replaced by more perfect deductions; but the observations always remain, partakers in final success. The feasibility of the works here proposed is undoubted. Their value can be safely predicted. This value will be enduring, not temporary.

But this project does not propose to build for posterity alone. It promises to lead to generalizations of immediate importance, and if the valuable deductions to which it may be fairly expected to contribute at once should fall short of expectation, it could only be a question of a few years before these could be realized.

The enterprise herein proposed offers a peculiar attraction in the powerful alliances of which it would form a part. It would form one of the converging lines which must result in producing what will prove to be the characteristic advance of astronomy in this century.

It will be natural first to consider the nature of the general problem in astronomy to the solution of which the establishment of the proposed observing station in the southern hemisphere may be expected to contribute.

Problem of the Sidereal System.

The mere appearance of the starry sky at night cannot fail to impress the reflecting mind with the thought that this vast aggregation of stars must contain within itself the evidence of organic arrangement. A more attentive examination, even without a telescope, while it reveals a general uniformity in the distribution of the stars

of various orders of brightness, also makes us aware that there are clusters of stars which point unmistakably to the operation of law. The vast aggregation of clusters making up the nebulous belt known as the Milky Way, which spans the sky as a kind of celestial equator, suggests orderly arrangement.

When we assist our researches with the telescope, merely as an optical appliance, evidences of such arrangement multiply until we are finally led to conjecture, by statistical methods alone, that the earth is situated within a vast cluster of stars very much more extended in the direction of the Milky Way than in other directions.

If we fortify our telescope with means of measurement we shall discover, after sufficient lapse of time, that some of the stars are in motion relatively to others. As we persevere in our measurements we shall discover an ever increasing number of stars partaking of this motion ; and we shall finally conclude that all the stars are in motion, some in one direction, and others in another. Some of these motions are only apparent. Disentangling these, after immense effort, we shall be able to recognize a peculiar drift of the stars, precisely like that which appears in surrounding objects when we are moving rapidly among them. This will finally prove to us that the sun is only a star, and that it is in rapid motion like all the others. Later we shall notice that separate groups of stars seem to be moving in a common direction like swarms of meteors ; and we shall begin to suspect that other evidences of law in these motions may be revealed to us at any moment if we persevere in our investigations.

Parallel with the discovery of these facts we shall be learning something of the distances of the stars. We are oppressed with the conviction that, so long as we are studying the arrangement and motion of the stars as objects upon a map, our information will be lacking in a vital element. We long to gain some conception of the space relations of the stars. From the very first we will have been led to suspect that some stars are brighter than others because they are nearer. When we attempt to test this hypothesis by measurement, we shall encounter the most formidable difficulties. Putting this difficulty in its simplest form it will mean that, if the vast area of the earth's orbit, more than one hundred and eighty million miles in diameter, were to be brightly illuminated and removed to the distance of the nearer stars it would appear as a mere point in the most powerful telescopes ; and to discover its dimensions would require the greatest refinement of skill even in the more favor-

able cases. Nevertheless, we shall be able to verify our conjecture in part, though we shall find that motion rather than brightness is the better criterion as to the distances of stars. Later we shall discover that we can test the relative distance of one large aggregation of stars from another, through one of the generalizations resulting from an investigation of the solar motion. Now, we shall have arrived at the point when we shall feel that we may some time see the stars in space of three dimensions—when we shall not only be able to make a flat picture of their relations as they appear to the eye upon the celestial vault, but shall also be able to construct a model showing the special and general relations in distance in space of three dimensions. Astronomy is standing at this point now. It takes some pride in what it has accomplished in this line already; but it is also more dissatisfied than ever with the amount of accurate knowledge which it has, because it sees how comparatively easy it would be to increase this knowledge in a very large ratio.

Again, parallel with these researches upon the motions and distances of the stars, there has been growing in importance another class of inquiries. Arming the telescope with a new appliance, the spectroscope, the evidence of universal motion among the stars, whether toward or away from the sun, has been most brilliantly confirmed. In this way the solar motion has been verified, and the approximate velocity of the sun in its flight through space has been determined.

Through its power of physical and chemical analysis the spectroscope has brought to light a collection of new facts of the highest importance in their bearing upon cosmical problems. It has shown the presence of terrestrial substances everywhere among the stars. It has demonstrated anew and more convincingly that the sun is a star, and even that there are multitudes of stars closely resembling it in their physical condition. It has shown us something of what is going on in the development of suns, or stars, revealing them at all stages of evolution and connecting one with another by imperceptible gradations from the newest to the oldest forms.

The unity of the stellar universe is thus demonstrated from three distinct points of view, and the conviction is brought home to us that there must be within our reach further and more interesting facts which will illustrate something of the arrangement and mechanism of this vast system of worlds flying hither and thither through space.

In outline this is what we shall term the *sidereal problem*, which

is now coming into view as the problem of the time, surpassing in the universality of its interest and in the grandeur of its scope all the physical inquiries which engage the attention of scientific men.

This problem should not be regarded, however, as synonymous with the whole range of sidereal astronomy. In the sense in which the term sidereal problem is employed here it would include those classes of research the chief interest of which centers in their bearing upon the description of the stellar system as a unit. There is another large range of extremely interesting investigations in the stellar field, in relation to which the chief interest centers in the natural history of individual objects. Even these may turn out to have their bearings upon the sidereal problem as a unit, but the connection is not so apparent. However much one class of researches may blend into another, the distinction here made may be found a useful one in the effort to give unity of design to the aim of the proposed Southern Observatory.

To unravel the problem of the solar system was the task which was first seriously proposed to astonomers three hundred years ago. Galileo, Kepler, Newton, and a long line of distinguished successors have successfully grappled with the intricate questions involved in that problem. Now the aspirations of astronomers are reaching beyond the boundaries of the solar system into stellar space. The stellar problem confronts us as the serious occupation of the present and the future, and it is illimitable in extent.

Need of More Astronomical Observers in the Southern Hemisphere.

Why should we go to the southern hemisphere in order to work upon this problem?

Were our object planetary research the inducement for another southern observatory might not be sufficient. All the planets can be seen from either hemisphere, and from either the whole fabric of planetary astronomy could be constructed without help from the other, though there would be undoubted advantage in cooperation.

But in stellar observation the case is different. Nearly one-quarter of the entire celestial sphere is inaccessible to exact observation from the observatories of the northern hemisphere. If we need observations of the nebulæ and stars in that part of the sky, we must go to the southern hemisphere to get them. For many of the most delicate researches fully one third of the sky should be under observation from the southern hemisphere.

Why cannot the observatories already in the southern hemisphere make all the observations that are needed there?

For this duty in the southern hemisphere we have about one tenth of the force which is available in the northern hemisphere for such observations—one tenth of the force to do one quarter or one third of the work. This point might be enlarged upon in detail, but it is so notoriously the fact as to make such illustration superfluous. The unanimous verdict of astonomers on this point may be derived from the correspondence which is transmitted with this report.

Nor does there appear to be any prospect now or within a reasonable time in the future that any material addition to the astronomical forces of the southern hemisphere can be anticipated through existing agencies. This ought not to be a matter of surprise. In addition to the numerous private, institutional, and municipal observatories in England, several astronomical establishments, including the celebrated Greenwich Observatory, are supported by the government in the British Islands. Therefore the British Government, in maintaining an astronomical observatory of high rank at the Cape of Good Hope, can scarcely be criticised for not doing more in that quarter of the world. Great Britain is the only one of the great powers from which support of astronomy in the southern hemisphere can be anticipated. Consider the local resources. We have the English colonies in Australia. Three small observatories are maintained there, of which, until recently, two have been almost exclusively concerned with meteorology. For somewhat more than forty years astronomical work has been carried on in the observatory at Melbourne, but the tendency recently has been to curtail this. Furthermore, the three Australian observatories have each recently undertaken a share in the Astrographic Chart, which is practically certain to absorb their entire energies for many years to come. Nor should the Australian governments be criticised because they maintain only three observatories on a small scale when we reflect that New South Wales, Victoria, and Western Australia combined have a population smaller than that of Texas. Still less should we anticipate any important contributions from the comparatively small communities in New Zealand, Tasmania, and South Africa. There remain only the governments of South America, and of these only two, Chile and Argentina, are in a geographical situation to become factors in consideration. Each of these countries maintains a national observatory. In the period from 1854 to 1862 the National Observatory of Chile, under a German astronomer, was active in a small

way. Recently a share in the Astrographic Chart was undertaken there, but Director Loewy informs us that this work has not been prosecuted there, and he expresses the hope that the Carnegie Institution may supply the vacancy. A strong national observatory was organized at Cordoba, in the Argentine Republic, in 1870, under the direction of Dr. B. A. Gould. This ranked among the leading observatories of the world for many years; but the financial disasters of Argentina have had a depressing effect upon that observatory, which, though it still continues its valuable work with perseverance and effect, is certainly in no position to undertake any additional obligations beyond that of a share in the Astrographic Chart and the other works there in progress.

Summing up, we find this situation: The Royal Observatory at the Cape of Good Hope will doubtless continue in its functions as an observatory of high rank, with the duty of doing for the southern hemisphere what Greenwich does for the northern as the primary object of its establishment. There planetary and stellar observations will be made, together with the observations that are required in the interest of government surveys. Over and above this we may fairly expect strong contributions from that observatory in a variety of fields. The remaining observatories of the southern hemisphere, with one exception, will be absorbed for at least 10 or 15 years in their work upon the Astrographic Chart, and, so far as can now be foreseen, will not be in a position to undertake much else of an important character. The one exception mentioned is the observatory at Arequipa. This observatory has been occupied in photometric and photographic researches of a nature similar to those carried on at Harvard College Observatory, of which it is a branch. These researches are of great interest and value, but they are outside the scope of the program here proposed.

Why Observations Are Needed in the Southern Sky.

It is desirable to consider more particularly, though briefly, why it is that the observation of objects in the far southern sky is so peculiarly desirable in connection with cosmical problems. If the problem were merely to ascertain whether the stars are in motion at all, or whether their motions vary with apparent brightness, or whether the distances of the stars are measurable in any case, these questions could be, as they have been, settled by observations on objects in the northern sky; but when we reach a point where

there is promise of fruitful generalization, the case may be different. It so happens that the various motions of the stars, real and apparent, are so involved that some means of separating their effects must be devised. Now, it happens that this separation is very difficult when the observations of one hemisphere are considered alone, while there is almost complete elimination of the interdependence of assumptions as to one class of motions upon those as to another when testimony is gathered from the entire sphere. To a very considerable extent this is true of the relation of precession to solar motion.

Again, different forms of a general rotation of the stars have been suggested. It is next to impossible to get any light on this question from the facts of observation relating to one hemisphere alone, because this effect is so entangled with the apparent motions caused by precession and solar motion that they cannot be accurately distinguished. With testimony from the entire sphere, a wrong supposition as to precession or solar motion would not so seriously affect the conclusion as to possible rotation, and in some circumstances scarcely at all.

Again consider the spectroscopic measurement of the velocity of the sun's motion through space. By combining the observations of both northern and southern stars we obtain the motion of approach contrasted with the motion of recession. If there should be anything in the suspicion that all the observations of velocity may be affected with some obscure source of error in common, this would be eliminated in the combination of the two hemispheres. In general it is obvious that we cannot neglect a large fraction of the sky when we are dealing with problems concerning the whole universe.

One consequence of this reasoning is that the proposed program for a southern observatory has this peculiar merit: that the results to be obtained are not only valuable in and for themselves, as constituting a needed increase in the sum of human knowledge, but they increase in a marked degree the value of the facts of observation already obtained in the northern hemisphere.

It does not seem at all surprising that our colleagues, with emphatic unanimity, should have expressed the opinion that additional astronomical effort at the present time can be expended to the best advantage in the southern hemisphere.

We have thus, here, an important research upon which further special investigations are needed, and in reference to which it ap-

pears very improbable that they will receive proper attention from other agencies within a reasonable time in the future.

Works of Observation Proposed and Instrumental Requirements.

We come now to the consideration of the particular works of observation which, in our opinion, could be undertaken to the best advantage by the proposed Southern Observatory. It is neither possible nor desirable that this one institution should undertake to make up all existing deficiencies in astronomical observation in the southern hemisphere. Other agencies have a responsibility in this field. New forces coming to the rescue ought to, and doubtless would, inspire the existing forces of astronomy in the southern hemisphere to still greater effort.

While not attempting to establish an inflexible criterion of selection for works of observation, we are of the opinion that the primary aim of the proposed Southern Observatory should be to make itself felt in the attack upon the sidereal problem as we have defined it—that it should seek to throw light upon the structure and mechanism of the stellar system as a unit. In other respects less attention need be given to those projects of observation that would suffer less by delay.

In preparing the schedule of proposed works we have been greatly assisted by the advice of astronomers whom we have consulted for this purpose. Their letters in response to our inquiries will be found in Appendix B, together with a preliminary statement of works proposed which forms the basis of the comments with which we have been favored. These comments have proved a most welcome assistance, and we have been strongly influenced by them in making up the schedule of proposed works.

(1) *Fundamental Meridian Observations.*

We regard meridian observations of precision, upon the brighter stars, to be of the first importance in any attempt to relieve the situation in the southern hemisphere. In this we are sustained by the nearly unanimous verdict of those whom we have consulted.

We are clear upon the proposition that the exact positions of about 6,000 stars (including all down to the seventh magnitude that are south of -20° of declination) should be determined by fundamental methods, both for the interest which this work commands as an inde-

pendent research and for the bearing it has on other works, including those here proposed.

In this line of work, as in nearly all other departments of astronomy, contributions from the southern hemisphere have always been deficient. With a few notable exceptions, there has been a lack of highly trained and experienced astronomers there. The recent efforts of the Cape Observatory in this line have been both skillful and energetic; but the Cape Observatory alone is unable to offset the numerous observatories in the northern hemisphere engaged in the precise observation of standard stars. The result is that the weight of our knowledge of the positions and motions of the standard stars in the northern sky is fully five times that for the far southern sky. A single corps of observers transferred from the northern hemisphere, where its loss would not be a relatively serious matter, to the southern hemisphere, where its services are so much needed in this line, could reduce the existing discrepancy of weight in fundamental determinations for the present epoch by one half.

It has been conclusively shown that the exactness of our knowledge of general drift in the motions of the stars, whether it arises from solar motion, rotation, or from any other source, depends almost wholly upon the number and precision of our fundamental determinations of the positions of the standard stars. The main battle is fought on this field. Furthermore, astronomy has now arrived at the point where, by comparatively little additional observation, it will be possible to compute the motion of nearly every star brighter than the seventh magnitude in the northern sky with a fair degree of accuracy. With the observations herein proposed, the same thing, in a modified degree, would be true of the stars in the southern hemisphere. Not many more than one third of these stars has been reobserved during the last quarter of a century. This work accomplished, astronomers could hope to deal successfully with problems of motion for all the stars visible without a telescope, and beyond that for all stars down to those which are one third as bright as the faintest visible to the ordinary eye—about 15,000 stars in all. This would give us the first opportunity ever offered for a comprehensive discussion of the solar motion and related problems on a scientifically correct basis, with a liberal supply of material distributed over the entire sphere.

This work would, therefore, possess a high scientific interest as an end in itself; but it would also serve as an indispensable basis for the observation of planets and of the fainter stars. A scheme

of observation like that described further on under (2) would require this work as its foundation. In fact, there is scarcely a single department of precise measurement in astronomy that would not be indebted to this work for a part of the data which it needs.

The problem of observation here suggested naturally divides itself into two sections. The first would be concerned with the observation by fundamental methods, and with the highest precision, of about 600 to 800 of the principal stars; the second would involve the extension of this work by less rigorous methods to about 5,000 other stars—the entire work to be conducted in such a manner as to be systematically consistent in all its parts, and to be a homogeneous whole.

There can be scarcely a doubt that the ideally best result would be attained through the adoption of the transit and vertical circle, for the observation of the principal stars at least. This, however, would prove somewhat more costly in execution, and the completion of the entire work, if it should be carried through with these instruments alone, would be deferred somewhat longer than might seem desirable.

The advantage of economy would attach to the use of a meridian circle for these observations. This would be increased, if the use of such an instrument that has already been thoroughly tested in the northern hemisphere could be procured, and we believe it can. The labor of a thorough investigation of the errors of graduation and other errors of such an instrument is, in itself, no slight task. What would be requisite for the purpose, here designed, would probably be equivalent to one full year of work by four observers. The degree of accuracy really attainable in the use of the meridian circle should not be sensibly inferior to that for a vertical circle. The distinct advantage of the latter is in the variation of method which it offers. For the present we should advise the employment of a meridian circle for this research, unless the proposed Southern Observatory should be established on a scale which would enable it to maintain for certain lines of observation the highest ideals.

Whatever the precise methods of observation may be, the instrument, or instruments, employed should be used for at least two years in the northern hemisphere in the determination of the positions of the principal fundamental stars visible there. There would result a peculiar gain in precision through the comparison of observations with the same instrument upon the same stars, made alternately in the two hemispheres, by which certain errors of the instrument and

a part of the uncertainties in our knowledge of astronomical refraction could be in a marked degree eliminated. The proof of this is already at hand in numerous comparisons of observations made contemporaneously with different instruments in the two hemispheres. From these it can easily be inferred what would be the probable gain if both sets of observations were to be made with the same instrument. We think that this plan presents a unique opportunity which ought not to be neglected.

If the meridian circle should be employed with a full corps of assistants the southern work here proposed could probably be accomplished within four or five years from the date of beginning. It would involve about 40,000 observations of about 6,000 stars and about 4,000 observations of stars north of —20° for the purpose of comparison and check. From four to seven observers and computers would be needed, with as many more routine computers, in order to keep the work going continuously and the computations up to date.

(2) *Complete Observation of Stars to the Ninth Magnitude.*

The exact determination of the positions of telescopic stars down to the ninth magnitude and south of —30° of declination is a work of high importance. The opinions of astronomers would differ as to the pressing nature of this work in comparison with the two succeeding works mentioned in this schedule. As will be seen from the correspondence transmitted herewith, there are opinions of great weight equally positive in favor of each of these undertakings. The plan for meridian observation of faint stars is mentioned second in order because of its intimate logical connection with the investigation already mentioned under (1).

The project to observe with meridian instruments the precise positions of all stars down to the ninth magnitude originated somewhat more than thirty five years ago, and was adopted as the peculiar function of the *Astronomische Gesellschaft*, which at that time assumed something of an international character. The importance of this project has been universally recognized. The original program for the northern hemisphere was completed under a cooperative arrangement. The extension of this work under the same auspices has been carried to —22° of declination, and the observatory at Cordoba has pushed the work on to —32° of declination. About one quarter of the sky remains to be considered. The com-

pletion of this one quarter would greatly enhance the value of what has been already accomplished.

The demand for this work may be stated under three principal heads:

1. The primary utility of this work would rest upon its immense importance in studies relating to the stellar system. The work mentioned already under (1) would enable us to study the structure and motions of the sidereal system so far as this is represented by stars to the seventh magnitude. The extension of this work by means of (2) would take us to stars of the ninth magnitude, for which the older records of observations contained observed positions of the greater part. We should then be able to extend our studies upon the sidereal problem to stars ten times as numerous and more than six times fainter than those of the seventh magnitude. The feasibility of reaping the full fruits of this undertaking through the present generation of astronomers is not so great as for the brighter stars, but that which would be demonstrably attainable now in this direction fully warrants the enterprise. Furthermore, the completion of this monumental work, so that the accurate position of every star from the north to the south pole, down to the ninth magnitude, would be known for epochs near the beginning of the century, would be an achievement upon which the entire civilized world could look with pride.

2. The positions of these stars determined in this work would possess very great value as reference points in all micrometric work upon faint stars, nebulæ, comets, and planets observed by means of extra-meridian telescopes. This alone was originally looked upon as fully warranting the labor of the entire enterprise.

3. This work would furnish the reference stars needed in the work of the Astrographic Chart, which has for its aim the determination of accurate positions for all stars down to the eleventh magnitude. This forms the third step in a series of investigations which seek to determine the accurate positions of stars at successive epochs in order that we may ultimately learn their motions. The accuracy of which this third step is capable depends upon the accuracy of its basis, which must be the meridian observation of telescopic stars. The only difficulty is that the accuracy of the observations hitherto made under the program of the *Astronomische Gesellschaft* is not regarded as sufficient for the purposes of the Astographic Chart, as Director Loewy has pointed out. He recommends as a substitute for (2) the special determination of the star positions required for

the Astrographic Chart. It will be found, however, that the so-called zone observation of telescopic stars may easily be brought to a very much higher grade of accuracy than that which has prevailed hitherto, without any material sacrifice of the rapidity with which they can be made. This can be effected in three ways:

(*a*) By the use of a superior instrument.
(*b*) By the use of a more accurate and more extensive standard catalogue, rendered possible under (1).
(*c*) Through a better organization in methods of observation.

Then with the addition of about one fourth the number of observations which would be needed under (2), without reference to any ulterior use, we shall be able to complete the *Astronomische Gesellschaft* zones and at the same time meet the requirements of the Astrographic Chart; and we strongly recommend that this program be adopted.

The necessity of this extended program is all the more pressing because of the extremely doubtful probability that the observations for the basis of the Astrographic Chart can be secured by means of existing agencies with the requisite completeness and accuracy.

The argument that the positions of all the stars down to the ninth magnitude (and fainter) in the southern hemisphere can hereafter be derived from the Astrographic Chart assumes that we may look for the completion of that undertaking within a few years. The probability that the completion of certain important sections of that work may be delayed for a longer time than would be desirable, however, seems to be warranted by an examination of the situation. On the other hand, it seems very desirable that the great undertaking entered upon thirty five years ago for the determination by meridian observation of the position of all stars down to the ninth magnitude in the whole heavens should be completed.

The proposed Southern Observatory has here an opportunity to carry out a work which would have enough of intrinsic interest in and for itself, but which would possess the great added advantage of serving as the fundamental basis of another work even greater in extent than itself. It would also much increase the value of similar work already accomplished in the northern hemisphere. Furthermore, it would scarcely be possible to carry out the work proposed next in order (3) in an economical, effective, and comprehensive way, as to stars in sensible motion, without the completion of (2).

For this work there should be a corps of five or six observers and

computers and a somewhat larger staff of mere routine computers. Under these arrangements it should be possible in a good climate to turn out at least 20,000 accurate star positions each year, and to make all the computations necessary to put them in catalogue form. Since not more than 200,000 observations would be required, the entire work should be completed within ten years from beginning—possibly in less time.

(3) *Measurement of Stellar Parallax.*

The determination of the distance of individual stars is one of the severest tasks in the science of observation. The results already obtained during a period of more than sixty years, but chiefly during the last fifteen, are not free from troublesome discordances that seem to encourage pessimistic views. Yet a review of the work which has been accomplished, together with that of the larger amount which is now in progress, affords great encouragement that we shall be able to determine average distances of classes of stars with a very satisfactory degree of precision. This is the important thing we need to know in our first studies of the structure of the sidereal system. The two great questions to be solved are:

(*a*) What is the relation of brightness to distance? Are the stars of the sixth magnitude as far removed on the average from those of the second as the relation of brightness alone would lead us to think? According to this relation, the sixth magnitude stars should be on the average rather more than six times as far removed from us as are the stars of the second magnitude. Is this really the fact? It is a question for measurement to decide.

(*b*) What relation to distance has apparent motion on the face of the sky? Are the stars which appear to move athwart the sky nine tenths of a second per year three times nearer us than the stars which move three tenths of a second per year? It seems very probable that this is approximately the case, and that apparent motion is a better criterion of distance than apparent brightness. Whether this conjecture is correct or not can be settled only by actual measurements of distances.

The decision upon these points is a fundamental necessity in the stellar problem, because it opens the way for a much more powerful and economical solution of questions in relation to distribution in distance through the discussion of meridian observations. That way is, to some extent, open now, but we need that certainty upon

the correctness of the fundamental principle (*b*) or of some modification of it which can only be had through direct measurement of the distances of a large number of stars.

That such measurements in large numbers are practicable is demonstrated by the successful work of Gill and Elkin at the Cape and of Elkin and Chase at New Haven with heliometers, by Kapteyn at Leiden and Flint at Madison (Wisconsin) with the transit, as well as by other observers.

Some of the available methods for measurement of parallax are:

By the use of heliometers.
By the use of meridian transits.
By photographic methods.
By micrometrical methods.

All these methods have been tried extensively; the first has apparently proved most accurate; the last, formerly employed almost exclusively, would probably be discarded now by common consent. Photographic methods have not hitherto proved entirely satisfactory perhaps, and yet it seems to be the almost unanimous opinion of those in the best position to judge that this method offers the greatest promise for efficient work on a large scale when properly used. The method of exposing the same plate at three successive phases of parallax, suggested by Kapteyn some years ago, is the one which seems to offer the greatest promise of economy in labor and precision in the result. From existing evidence it does not appear that the photographic method is likely to be very effective upon stars brighter than the fifth magnitude.

The heliometer would probably be better suited for parallax measures of bright stars. This method, though extremely precise, is slow and costly. It has been employed at the Cape of Good Hope in measurements upon a few of the far southern stars; but, so far as is known, no work of the kind is now going on in the southern hemisphere. The Cape heliometer is now devoted to planetary observations upon a new plan, a work very appropriate to the original purpose of the Royal Observatory at the Cape of Good Hope. Therefore, with the exception of the aid which might hereafter be rendered by the Cape heliometer, the entire field for the determination of stellar parallax in the southern hemisphere is open to the Carnegie Institution, should it desire to enter it.

This work could be undertaken with advantage on almost any scale. Without any idea as to what means might possibly be avail-

able, we are unable to suggest a definite program other than the restricted one which follows.

In the first line it seems desirable that an attempt be made to measure the parallaxes of stars known to be in sensible motion. As the limit of such motion for a comparatively restricted work, one might take 0."2 or even 0."1. In order to identify the stars having such motions, and to measure the motions themselves, the sources of information are now very scanty. It would be almost a necessity to carry through (2) of the program—the meridian observation of telescopic stars down to the ninth magnitude. If this were not done it would still be necessary to make accurate meridian observations of all stars observed for parallax. The number which would be at present available under this plan and south of —20° of declination would probably be considerably under 500, though others could doubtless be found without much difficulty. We would recommend the use of a photographic telescope of relatively long focal distance for this purpose. It should be of the highest optical perfection. It is quite possible that a three lens or four lens combination would be advisable, in order to get sharp, round images over an area of at least four square degrees if possible. We suggest a telescope of 18 inches aperture and about 30 feet focal length.

It would also be highly desirable to measure with the heliometer the parallax of many stars brighter than the sixth magnitude and south of —20° of declination. A seven inch heliometer similar to those in use at the Cape and at New Haven would be suitable for this purpose. The stars selected should be chiefly those distinguished for proper motion—several hundred in number. This would be in effect an extension of the parallactic survey which has been carried on so successfully at New Haven within recent years.

We think that the services of two observers on this work for a period of at least eight years would result in an extremely valuable contribution to the solution of the stellar problem.

For some years the project of a general *Parallax Durchmusterung* has been brought to the attention of astronomers. This contemplates nothing less than the determination of the approximate relative parallax of every star down to the ninth or tenth magnitude. The inquiry would mean something like this : If we take as the unit of distance the average star of the ninth magnitude, what are the relative distances of other classes of stars, and what individual stars are especially near the solar system ? A program for this purpose is described in the letter which Professor Kapteyn has addressed

to the Committee in response to its request (Appendix B). This would be an immense undertaking, which would seem to demand the cooperation of several agencies before it could be properly undertaken for the entire sky. It would also be desirable to learn more of the practical capabilities of the method proposed before making any recommendations upon the subject.

The plates which would be taken in the course of the attempt to determine the relative parallaxes of stars known to have sensible motion would themselves offer an opportunity for preliminary tests of the method and of the value of the expected result. It would also be easy to provide this in a more systematic form for restricted areas of the sky by the exposure and development of plates especially for that purpose, provided the measurements and computations for these plates could be arranged under a cooperative plan of limited extent, such as that suggested in the letter of Professor Kapteyn (Appendix B).

In view of these considerations we are of the opinion that it would be advisable, in connection with the proposed Southern Observatory, to set up a photographic telescope of about 18 inches aperture and 30 feet focal length for parallax work on the southern stars, and that it would probably be found desirable to maintain it in constant operation for a period of eight years at least. The services of two skilled observers and a small staff of measurers and computers would be required.

(4) *Measurement of Radial Motions.*

In close connection with the three projects already mentioned is another for the measurement of velocities of stars in the line of sight—*i. e.*, radial velocity toward or away from the earth. A knowledge of these velocities is of the utmost importance, and since the accurate measurement of such velocities has become possible, a most valuable source of information for verifying and enlarging the conclusions to be drawn from the discussion of proper motions has been placed in the hands of astronomers. In fact, through an adaptation of the investigations for solar motion in connection with these measurements of radial velocity, it is possible to obtain valuable conclusions as to the distances of various classes of stars having sensible proper motions. When a very large number of such measurements upon stars distributed over the entire sky shall have been obtained, it will be possible to determine the velocity of the solar motion with great precision. It will even be

possible to obtain in this manner an absolutely independent check upon the direction of solar motion which, in a problem so important, will possess the highest philosophical value and will become a valuable test of fundamental hypotheses as to structure and motion in the sidereal system.

Whether the stars are distributed with approximate uniformity in volume, and whether the motions are at random in every conceivable direction, are questions which require for a definite decision the added information which can be obtained by the measurement of radial velocities of large numbers of stars extending, if possible, somewhat below those visible to the unassisted eye, in combination with discussions founded on proper motions which result from meridian observations. What we now most need is such measurements for stars not visible from the observatories of the northern hemisphere. Scarcely anything in this line has been accomplished for the southern hemisphere. Recently the Lick Observatory has dispatched an expedition to Chile with the Mills three foot reflector to make such measurements, and the maintenance of this expedition has been provided for by Mr. D. O. Mills for three years. It may be expected to produce results of the highest importance—of importance relatively several times as great as would attach to like efforts with the same instrument in the northern hemisphere.

Thus we may hope to have at our disposal within a few years the measured radial velocities of practically all the stars in the whole heavens that are brighter than the fifth magnitude. This will be an extremely valuable result; but it would be made far more valuable if such measurements could be secured for a greatly increased number of stars over a greater range of magnitude.

Are the velocities of the more distant stars the same on an average as those of the nearer stars? Are the peculiar motions of the stars, after abstraction of parallactic motions, the same in the directions to and from the observer at different distances from the Milky Way? In order to answer these and similar questions with sufficient weight of evidence, the objects for which velocities in the line of sight have been measured should number 2,000, if possible; and in order to accelerate the rate at which results can be reached, we need telescopes of the largest possible light-grasp. There is no apparent obstacle, except cost, in the way of employing a telescope with a five foot mirror for this investigation.

That an increase in the resources for the measurement of radial velocities of stars in the far southern skies is desirable, appears from

the fact that, while there is permanently located in the southern hemisphere only one telescope which is used for this purpose, and provision made for the use of another during three years, there are employed in this service in the northern hemisphere at least six large telescopes, each more powerful than any telescope installed at a permanent observatory in the southern hemisphere. The disparity in resources here presented is marked, and the call for a remedy seems to be imperative.

It would therefore seem to be very desirable that the Carnegie Institution should enter this field and provide for use in the southern hemisphere the most powerful reflecting telescope that would be sanctioned by experience and the dictates of common prudence. The use of this in the measurement of radial velocities of southern stars should be provided for during six years at least. There would be needed a staff of two skilled observers and a half dozen measurers and computers.

The additional argument in favor of the provision of a large reflector to be used in the southern hemisphere will be found later, under (7), in this enumeration of proposed works.

The observations specified under the four preceding heads are closely related to each other, and logically they are branches of a single enterprise—an endeavor to make a strong forward movement in the solution of the sidereal problem. We consider it extremely desirable that provision for all the works enumerated by us should be made in the proposed Southern Observatory, but in the event of necessary curtailment which does not extend to the entire program, it is to be hoped that such curtailment may not apply to either of the four projects thus far mentioned.

(5) *Observations of Double Stars.—A Large Refractor.*

The measurement of double stars has been in active progress for more than a century. For the past seventy years work in this line has absorbed a very large proportion of the energies devoted to astronomical investigation. Certainly the class of facts developed by these investigations have remarkable interest, and they have exerted a deep influence upon the thoughts of man as to his place in nature. That the law of gravitation apparently extends throughout the universe; that suns revolve about suns; that the orbits are usually elliptical, like those of the periodic comets; that many of these bodies are larger and more splendid than our sun—these and

numerous other facts seem to lend a special significance to this branch of astronomy. Except incidentally, this work is not very closely associated with the stellar problem, and the immediate advantage of a greater extension of work in this line in the southern hemisphere is not so apparent; yet it should be remembered that no really large telescope has yet been applied for any great length of time to the measurement of double stars in the southern hemisphere, and while we put this project after the four already mentioned, we entertain no doubt of its desirable character.

It would seem very desirable that a telescope of about 27 inches aperture should be provided for this work. There should be a regular survey of the entire southern sky for the discovery of new double stars, to complete similar surveys carried out at the Lick Observatory and elsewhere.

Double-star work on the southern sky has come practically to a standstill, while it is still going on industriously at several observatories in the northern hemisphere. That a large telescope in the southern hemisphere should be devoted to double-star measurements for a period of about eight years seems to be evident.

One observer and one assistant would be required.

If a large refracting telescope should be provided for the proposed Southern Observatory it is very likely that certain micrometrical and other studies, apart from double-star observations, would be worth while, perhaps calling for the detail of another observer. This comparatively small expenditure would doubtless be well compensated in the increased utility of a costly telescope justified for another purpose.

Possible combination of (3) and (5).—In providing the instrumental means for carrying out (3) and (5) upon a restricted scale, it might be possible to combine the two telescopes required, so that one mounting and observing room would suffice. This would result in a very large saving in plant. The tubes of the two telescopes, each designed as perfectly for its peculiar purpose as if made for special instruments, could be attached to the same declination axis. Parallax plates should be exposed in the early evening and late morning hours, in any case. The result would be that the parallax telescope would usually be unused during the four or five hours around midnight, in order to avoid the use of the telescope at large hour angles east or west. During this period, if the parallax telescope were combined with the telescope for micrometrical observations the latter would be wholly free during four or five hours near midnight, the

hours which, on the whole, would be most suitable for the observer of double stars.

This arrangement is not suggested as ideal, but as one which might be adopted should the program for (5) be carried out and that for (3) be somewhat restricted. In that event we think the arrangement here suggested would be very much preferable, as it certainly would be very much more economical.

(6) *Variation of Latitude.*

The variation of the earth's axis of rotation is one of the newest and most interesting developments of astronomy. The facts relating to it have an intimate bearing upon the question of precision in meridian observations, in addition to the great interest which attaches to it as a physical phenomenon. Astronomy has not yet formed an adequate explanation of its origin. Quite recently Mr. Kimura, of the Japanese international latitude station, has called attention to a singular phenomenon developed by the international observations, which may be referred to several possible explanations, none of which appear quite satisfactory. In consequence of this, Dr. Chandler, editor of the Astronomical Journal, points out the necessity for establishing three observing stations in the southern hemisphere—one at the Cape of Good Hope, another at Sydney, and still another about 30 miles south of Santiago de Chile. This proposition has been heartily indorsed by several high authorities, including the Royal Astronomical Society. The operation of these three stations would tend to fix and define this anomaly in the observations beyond a doubt, and the result might be that the true cause of the phenomenon in question would be pointed out. Furthermore, the execution of this project would bring a most valuable contribution to the question of astronomical aberration.

We are strongly impressed with the importance of this work and are of opinion that, if the stations at the Cape of Good Hope and Sydney could be provided for by other agencies, the Carnegie Institution would do well to take the responsibility of the station in Chile. The annual expenditure would not be very great, and this project need not stand or fall with that for the Southern Observatory, although it has a logical connection with it.

The zenith telescope and observing shed required would not be costly, and two observers, without other assistants, would be able to take care of the observations and computations.

In addition to the undertakings we have already enumerated as offering a definite field of great usefulness for the proposed Southern Observatory, there are others which, in the judgment of many astronomers of high standing, deserve a prominent place in this enumeration. The following list briefly recapitulates some of these.

(7) *Astrophysical Researches.*

Should a large reflector be provided for the observatory in connection with (4) it could also be utilized with advantage for certain photographic and astrophysical researches of great importance to the advance of science. Among these are:

(*a*) Spectroscopic researches upon red and variable stars to supplement similar researches in the northern hemisphere.

(*b*) Photography of nebulæ in order to make the record of similar works complete for the entire sky.

(*c*) Spectroscopic examination of the nebulæ for the purpose of ascertaining their motions in the line of sight, as well as in the interest of inquiries into their physical nature.

These and similar works are named as supplementary to that mentioned under (4), not to be carried on to the detriment of the latter.

(8) *The Astrographic Chart.*

Allusion has already been made to the labors upon the "Carte du Ciel," or Astrographic Chart. Director Loewy, president of the Astrographic Congress, informs us that there is a vacancy in one of the zones; and he states that the services of the Carnegie Institution would be very acceptable in filling the vacant place. There is no question of the importance of this great project for securing a photographic representation of the entire sky at the present epoch. The work has already progressed so far that its ultimate success is now practically assured. It would be most unfortunate should the completion of one or two sections be delayed far beyond the completion of all the others, thus destroying the unity of epoch that is so desirable in all such works. This work requires the use of a thirteen inch photographic telescope of a special type. This instrument would not be very costly. The services of two observers and of a small corps of measurers and computers would be required during about eight years. We think that the feasibility of undertaking

this work ought to be taken into serious consideration by the Institution, should it be ascertained that there is no reasonable prospect of provision for it elsewhere.

(9) *Photometric Observations.*

Professor Seeliger urges the importance of precise photometry on the southern stars in the interest of problems relating to the structure of the sidereal system, and to supplement similar work carried on in the northern hemisphere. All the arguments which have been presented in relation to the first four numbers of this program apply to this. Statistical methods of investigation in this field have already led to significant and interesting conclusions, and we cannot doubt the power of this method, which has already proved such a valuable guide in affording reliable clues to the structure of the sidereal system. We think that Professor Seeliger's suggestion is worthy of further inquiry and consideration.

(10) *Researches of Minor Scope.*

A number of researches of minor scope have been suggested by our correspondents, all of which are of importance. For some if not all of them it seems desirable that interest should be excited at existing observatories in the southern hemisphere to take up these works and carry them to a successful conclusion. One of the good results which we should hope from the proposed Southern Observatory would be that it would serve to stimulate interest in astronomy throughout the populations of the southern hemisphere.

It will thus be seen that there is no lack of work of pressing importance to be done in the southern hemisphere. If an observing station should be established on a liberal scale for the execution of these works, it would still be a problem requiring wisdom and firmness to keep the program within practical limits and concentrated upon the furtherance of the great end desired.

THE QUESTION OF SITE.

We have given much attention to the question of a suitable site for the proposed Southern Observatory, but we are not yet prepared to make a definite recommendation.

For valuable information in regard to this question we are indebted to Mr. H. C. Russell, Government Astronomer at Sydney,

New South Wales; Mr. W. Ernest Cooke, Government Astronomer at Perth, West Australia; Dr. John M. Thome, Director of the observatory at Cordoba; Mr. Walter G. Davis, Director of the Argentine Meteorological Service, and Sir David Gill, Astronomer Royal at the Cape of Good Hope. We are greatly indebted to these gentlemen for the painstaking and valuable information with which they have favored us, for cordial offers of facilities, and for documents of interest which they have forwarded.

We have also devoted study to the meteorological and climatological reports in regard to the countries crossed by parallels of latitude suitable for the location of the observatory. In a preliminary way it appears to us that the most promising localities are in New South Wales, in the vicinity of Sydney; in South Africa near Bloemfontein, or on the Great Karoo plateau in Cape Colony, and near San Luis, in Argentina. San Luis appears to have a very clear sky and a salubrious climate. It is only 16 hours by rail from Buenos Aires. It is measurably free from the fearful "hondas" or stifling hot waves which characterize the Andean plateaus further west, and the skies are remarkably clear.

The latitude of Bloemfontein is rather smaller than is desirable, only 29° south, while —30° would seem to be almost the northerly limit admissible. Yet we have from Sir David Gill, who has inspected that locality in connection with the trigonometrical survey, the most favorable accounts of the wonderful transparency and steadiness of the atmosphere there, and of the remarkable number of clear nights, which is estimated at 300 annually, or about three times the number we experience upon the Atlantic seaboard. The elevation is about 4,000 feet above sea level. The mean annual temperature is, however, rather high.

Our early reports in regard to Australia were favorable, and we decided to procure a careful test of certain sites in the vicinity of Sydney. In April of the present year Professor W. J. Hussey, of the Lick Observatory, was appointed to make telescopic tests and other examinations with reference to observatory sites. He was engaged up to the end of July upon explorations in southern California, looking for a site for the Solar Observatory. Soon after the completion of this work he sailed for Sydney, on August 6, under instructions to test certain sites in the neighborhood of Sydney in relation to which we had formed favorable opinions, confirmed by the personal testimony of Mr. Russell. Mr. Hussey is provided with an excellent portable telescope of nine inch aperture, of which

the object glass was most obligingly loaned by The Alvan Clark and Sons Corporation of Cambridge, Mass., and of which the principal parts of the mounting were also loaned by the Lick Observatory, thus reducing the expense of the telescope to a nominal sum. With this telescope the character of the "seeing" is ascertained by systematic tests at all the stations visited. This can be compared with the excellent conditions prevailing on Mount Hamilton, where Mr. Hussey has had many years' experience.

Pending Mr. Hussey's examination and report, it scarcely seems worth while to enter into a detailed discussion in regard to sites at the present time. If the proposed Southern Observatory should be organized upon a scale sufficient to secure the execution of the greater part of the works enumerated in our program, the question of site would be very important indeed, and we have so regarded it. It is essential that the climate should be healthy for astronomers engaged upon tasks so strenuous; that there should be a large proportion of clear nights; that the air should be reasonably transparent and exceptionally steady, and that, so far as possible, the ordinary comforts of civilization should be found in the environment of the observatory. In the search for a stimulating and healthful climate, and also in the interest of the meridian observations, it would be desirable to choose a latitude of 40° south or more.

But in the southern hemisphere, in latitudes south of —35° or —40°, the amount of cloudiness is apt to be very great, or the climate is otherwise unsuitable. In other respects, as in what was formerly called Patagonia, some of the localities in high southern latitudes cannot be regarded as available. The vicinity of Hobart Town, Tasmania, offers many advantages, especially in the healthfulness and uniformity of its climate, but with the disadvantage of rain falling on nearly half the days of the year. The amount of clear weather in New South Wales, though superior to that of our Atlantic seaboard, is not quite all that could be wished. The object of the proposed observatory is to secure valuable observations in large masses by the expenditure of great energy during a comparatively short term of years. This qualification is not the only one, however, and may possibly be regarded as compensated somewhat if a locality can be found where the climate is healthful and where efficient routine assistants can be recruited from the surrounding population.

The funds appropriated for the use of the Commission will suffice for what it has undertaken to accomplish; but if site explorations

in South Africa or Argentina are desired, a further small appropriation will be necessary. Later, through the operations of Mr. Hussey and the kindness of Sir David Gill, the Committee expects to be in possession of better knowledge as to what may be advisable in this direction.

BUILDINGS.

The question of buildings and other constructions necessary for the proposed observatory is one which cannot be discussed in its minute details until something shall have been determined as to the site. Since the idea of this observatory is that of a temporary observing station, to be occupied, perhaps, not more than ten or twelve years, our ideas of the construction required would be largely controlled by that fact.

The necessity of providing for the equalization of the inner and outer temperature seems to prescribe for the observing rooms a form of construction which would not be very different whether the observatory were to be temporary or permanent. The essential principle is that the walls should consist of an iron or steel framework, with an outer covering of wood, in the form of louver work, and an inner covering of sheet metal, such as galvanized iron. The efficiency of this form of construction has been fully tested and seems to leave nothing further to be desired. For the drum to carry a large dome, this form of construction would probably be as economical as any other that could be accepted. Wood might be used for the framework were it not for the necessity of an even and solid construction for the tracks upon which the rolling mechanism of roofs and domes is supported. For the meridian instruments, sliding roofs should be provided. The great superiority of these over the old form of shutters is now fully demonstrated.

Since an important requirement for site is excellence of atmospheric conditions, it follows that the proposed observatory must be located at some distance from any large center of population. Consequently it would be practically unavoidable that provision should be made for housing the observing staff upon the observatory premises. This is an arrangement which is quite indispensable to the highest efficiency in any case. In any but an extremely exceptional climate there will be a large proportion of nights in which the probability of clear sky during the first half of the night will be doubtful. If observers live within easy access to the instruments, much clear sky will be utilized that will inevitably be lost otherwise.

Furthermore, after long duty at the instrument the observer is in need of rest, and in order to attain it should not be subjected to the hardship of a journey on foot for a mile or two in the small hours of the morning. For those observers who are obliged to begin duty at some time after midnight, it is practically indispensable that their residences should be in close proximity to the instruments they are to use. Aside from these obviously practical considerations, it has been found by experience that the plan of housing the staff upon the observatory premises effects a real economy in the quality and quantity of output in relation to the total expenditure.

We are of the opinion, therefore, that residences for the observing staff should be provided upon the site of the observatory. All that would be needed would be small cottages of simple construction, suitable to the climate. Equal simplicity ought to prevail in the construction of the office buildings required for administration, computing, library, and storage. Small work rooms would also be needed for the mechanical department.

Staff and Organization.

The question of organization for the proposed observatory is one which cannot be effectively discussed until something shall be known of the definite purposes of the Trustees in relation to its establishment. It goes without saying that no work whatever should be undertaken which cannot be put under the direction of assuredly competent and energetic astronomers interested in what they are to undertake. If the scheme should be inaugurated in its broadest scope, as we hope, it would be a mistake to begin the execution of the plans until the general control and direction can be arranged in a manner to command the confidence of the astronomical world, as well as that of the Trustees.

In some respects, however, the situation would be peculiar. In the ordinary case one of the most important functions of the directof of an observatory is the control which he exercises in the choice or work and methods. In the present case the director would be partly shorn of this privilege at the outset, since the very idea of the proposed observatory would be the performance of certain definite tasks.

Furthermore, in the selection of the staff for the execution of the full program it would be necessary to select, in addition to routine assistants, about twenty astronomers and assistant astronomers of proved capacity and experience in varying degrees. Other qualifications, such as health, energy, and capacity of adaptation to new

surroundings, would have to be considered in an unusual degree To recruit this staff all at once, for temporary service, from the existing forces of astronomy might prove to be a somewhat difficult task. In case this project should be adopted, therefore, the Institution should be prepared for a somewhat gradual organization of the observatory, extending possibly over four or five years, before all its departments should come into full action.

This delay, however, might prove unavoidable from another point of view. The necessary provision for instruments and for their proper installation on so large a scale is a matter which cannot suitably be disposed of all at once. In the light of previous experience it may be estimated that a period of three or four years, at least, would be necessary before the means for observation could be prepared in all its details.

The Southern Observatory as an Expedition.

An important part of the astronomical work of the southern hemisphere has been the result of special expeditions. There are the early expeditions of Halley and La Caille and the later ones of Sir John Herschel to the Cape of Good Hope; of Johnson to the island of St. Helena; of McClean to the Cape of Good Hope for astrophysical work, and of others. The expedition of Captain Gillis to Santiago de Chile in 1850–'53 resulted in several extensive series of astronomical observations. The establishment of the Argentine National Observatory originated in what was essentially an astronomical expedition of the most fruitful character under the conduct of Dr. B. A. Gould. The extremely valuable work of Stone at the Cape of Good Hope was also virtually that of an expedition for a particular purpose. Nearly all the other astronomers who have done highly valuable work in the southern hemisphere have been northern astronomers who went to southern stations for some special work.

The idea of a temporary observing station in the southern hemisphere will not, therefore, seem to be in any way strange. In one respect it embodies an extremely economical principle—the observatory would be maintained only for the accomplishment of works deemed highly important. There would be no chance for it to pass through stages of comparative inaction or to engage in work which is comparatively less pressing, or in undertakings equally well done elsewhere. These are the dangers that may threaten a permanent institution.

Furthermore, the duties of administration would be somewhat simplified. It would chiefly be necessary to provide the means for ascertaining whether the works adopted are pushed with the requisite energy and skill—whether the product is that which was stipulated.

Pushing this idea to an extreme, the work of the observatory would virtually consist of a series of expeditions having scarcely any connection one with another, except that of proximity at the scene of operations. As fast as the preparations for one of these lines of work should be complete, the expedition for that would be despatched upon its mission, with its own head and its own staff.

The larger the establishment, the more necessary it would be to provide a strong observatory organization.

All these questions can be more effectively studied after it becomes known upon what scale the enterprise can be carried out.

III. Observatory for Solar Research.

As the central body of the solar system, confining the planets in their orbits by the power of its attraction and supplying them with light and heat through its radiation, the sun possesses for us an interest greater than that of any other celestial body. From one point of view this interest may be considered to be of a most practical character, since the conditions of terrestrial life are determined exclusively by the solar radiation, so that any possible changes which this radiation may undergo are likely to be of consequence to life upon the earth. From another standpoint the study of the sun possesses a philosophical interest of the highest kind, for the sun is a star, comparable in all particulars with countless stars which lie beyond the boundaries of the solar system, but possessing the unique distinction, through its proximity to the earth, of being susceptible of detailed study and investigation. Thus in all reasoning on the physical constitution of the stars, especially in connection with the great problem of stellar evolution, we must start from the sun as a type object and elucidate stellar phenomena from an intimate acquaintance with solar phenomena. We have no foundation for the hope that any other star will ever appear larger than a microscopic point of light, even though the telescopes of the future may completely outrank the instruments of the present day; but through the provision of more adequate means of studying the sun

we may hope immeasurably to strengthen and deepen the foundations on which investigations of stellar phenomena are laid.

Conversely the only means of studying the origin and development of the sun and of determining what it will become in the future is afforded by the phenomena of stars and nebulæ, for we find in the heavens stars in all stages of growth, illustrating every step in the process of evolution by which the sun has been developed from a nebula. Solar research should thus begin with the nebulæ, proceed with a physical investigation of those celestial objects which represent the earlier stages of stellar growth, culminate in a study of the solar structure and radiation, and conclude with an examination of the red stars, one of which the sun will some day become. The study of the sun, with the inseparably connected question of stellar evolution, thus presents a single great problem, important alike to the philosopher, the astronomer, the physicist, the chemist, the geologist, and indeed to every one interested in the study of nature.

Purpose of a Solar Observatory.

After full and careful consideration of the recommendations of the Advisory Committee on Astronomy * and an extended examination of the various questions involved, we respectfully recommend the establishment by the Carnegie Institution of a Solar Observatory, so situated and equipped as to permit the accomplishment of three principal objects:

(1) To measure the intensity of the solar heat radiation, and to determine whether it varies from perfect constancy during at least one sun spot period of eleven years. In connection with this investigation, to measure the absorption of sunlight in its transmission through the atmosphere of the earth and that of the sun, and also the radiation of different portions of the sun's image, such as spots, faculæ, and prominences.

(2) To bring to bear upon the solution of solar problems various modern methods of research, principally of a spectroscopic nature, which have not hitherto been applied with adequate facilities. More specifically, to provide for the investigation of various solar phenomena with the spectroheliograph, the visual and photographic study, with powerful spectroscopes, of the spectra of the chromosphere, sun spots, and for other researches of a similar nature.

*Carnegie Institution Year Book No. 1, Appendix A, p. 96.

(3) To provide, through the construction of a large reflecting telescope, for the investigation of various problems of stellar evolution, intimately related to solar work, which existing instruments are inadequate to solve.

It will be seen that the investigations here proposed may be grouped in another way, viz., (1) those which relate to the sun's radiation, mainly with reference to its effect upon the earth; (2) those which relate to the solar constitution, with special reference to the sun as a typical star; and (3) those which relate to the evolution of stars like the sun from nebulæ.

There are many important reasons to recommend the establishment of a solar observatory by the Carnegie Institution. Up to about the year 1875 a large amount of information regarding the phenomena of the sun's surface had been collected, partly through the utilization of more and more powerful telescopes, and particularly through the recent application of the spectroscope. But since that time, for reasons not easily to be explained, comparatively few important advances in the study of these phenomena have been made; very little advantage has been taken of the great improvement in telescopes and in spectroscopes during the intervening quarter of a century. No other department of astrophysical research has been equally neglected, and consequently in none is there such an exceptional opportunity for great advances. Only one of the twenty two refracting telescopes of from 20 inches to 40 inehes aperture is regularly used for work on the sun, and with but two or three exceptions the solar spectroscopes in use are little better than those of a quarter of a century ago. Though such spectroscopes are fairly well adapted for the statistical work in which they are employed, they are wholly incapable of dealing with phenomena easily within reach of such spectroscopes as are used in physical laboratories. Even in the few cases in which important advances in solar investigations have been made, partly through the invention and perfection of new instruments, the means available have generally been inadequate to bring out the full powers of new methods of research, and atmospheric disturbances have always most seriously hampered observation. What is needed is an observatory at some suitable mountain site, where atmospheric disturbances are reduced to a minimum; the development of special forms of telescopes, particularly adapted to solar work, and the complete utilization of the numerous improvements in spectroscopes and other instruments for physical research which have been developed in the

physical laboratory and require laboratory conditions for their successful use. No existing organization proposes to do this work under these conditions, and there is no prospect that it will be undertaken unless by the Carnegie Institution.

Advantages to be Gained Through Improved Atmospheric Conditions.

Up to the present time practically all observations of the sun have been made from the lower regions of the atmosphere. This surrounds the observer in a vast fluctuating mass, which reduces the brightness of the heavenly bodies by nearly one half, and transforms their images, which should be sharp and clearly defined, into boiling and confused objects, in which the delicate details of the originals are almost wholly concealed. At rare moments of comparative calm, glimpses may be had of structure of indescribable delicacy, but if partially revealed for a moment it is instantly swallowed up by disturbances in the atmosphere. It is as though the astronomer were forced to make his observations from the bottom of an ocean, whose constant storms are not confined to its surface, but penetrate the utmost depths, churning them into a seething mass, through which all external objects seem vague and ill defined. It is evident that such disturbances in our atmosphere must prevent not only a clear and perfect understanding of the solar structure, but in no less degree an accurate and reliable measure of the intensity of the solar radiation, which will seem to vary with the fluctuations in the atmospheric absorption.

A sharp distinction must here be drawn between two very different kinds of disturbances which the atmosphere produces.

(1) In the measurement of the sun's heat radiation, to determine whether it varies from year to year, the *absorption* of the atmosphere is the principal obstacle. No solar image is required, and local disturbances due to irregular refraction are of little consequence. The absorption may evidently be obviated in large part by making observations from the summit of a very high mountain, at a point well above the denser portion of the atmosphere where most of the absorption occurs.

(2) The detailed study of the various phenomena of the sun's surface, on the other hand, is impossible without a large and well-defined image, free from disturbances caused by local inequalities of temperature in our atmosphere. Currents of warm and cold air, especially if they are in the neighborhood of the instruments, are fatal to successful work.

The absorption of the atmosphere for heat and light radiations depends mainly upon the length of the air path which must be traversed by the rays. At an altitude of 15,000 feet the most fluctuating part of the atmosphere has been left below, and the rare atmosphere above is subject to comparatively little variation in its absorptive power. If, then, an observatory for the study of solar radiation should be established at some such height above the earth's surface, in a region where but little water vapor is present, the difficulties hitherto experienced in the measurement of the solar heat would in large measure disappear.

But it by no means follows that such a site would be suitable for that department of solar research which requires a perfectly defined image of the sun. As a matter of fact, the sharpness of definition experienced during the day on mountain tops is frequently much inferior to that which may be found at lower levels. At the summit of Pikes Peak (14,147 feet), for example, although the transparency of the atmosphere is very marked, the sun's image is usually not well defined. The same is true at Mount Ætna (9,650 feet), except in the early morning when the low sun has not yet greatly heated the mountain slopes. At the Lick Observatory, on Mount Hamilton (4,208 feet), the day conditions are better, and are probably similar to those which are found at the Yerkes Observatory (1,100 feet).

Until recently no mountain peak has been known on which the sun could be observed to advantage throughout the day. This is presumably due in large part to the fact that the mountain slopes, if not thoroughly covered with foliage, become greatly heated by the sun's rays, producing ascending columns of warm air, which rise toward the summit and mingle with the cooler currents brought by the wind. Under such circumstances bad definition would be inevitable. At Mount Lowe (5,650 feet), near Pasadena, California, in a region where the remarkable uniformity of temperature and pressure would lead one to expect good definition, the solar image is frequently disturbed by currents of warm air rising from the unprotected slopes. Separated from Mount Lowe only by the width of a single canyon is Mount Wilson (5,886 feet). This mountain is well covered to the very summit with foliage, and thus stands in marked contrast with many of the mountains in southern California.* In consequence of this fact, all of the advantages to be expected from the exceptional quality of the atmosphere are experienced, without the disadvantages due to warm air rising from the heated

* See Professor Hussey's report, Appendix A.

slopes of the mountain. It is not surprising, therefore, that the sun's image, as seen from the summit of Mount Wilson, is apparently better defined than at any other point hitherto tested with a telescope.

But for the successful prosecution of solar research another condition must be fulfilled. The phenomena on the sun's surface are constantly changing in form, not only from hour to hour, but from second to second in the violent eruptions which are numerous during the period of greatest solar activity. In order to study these changing phenomena intelligently, it is necessary that they be kept constantly under observation. In observatories subject to frequent clouds and storms the progress of such solar changes cannot be steadily watched. At some critical moment clouds frequently interpose to prevent further work. In an investigation of the solar rotation, for example, it is of great importance that the position of a sun spot or a facula be determined day after day without interruption. In actual practice many of the photographs made at existing observatories are rendered almost useless on account of the cloudy periods which separate them from other photographs. Mount Wilson has the unique advantage of combining extraordinary perfection of definition with such freedom from clouds as to permit continuous work for months at a time.

Summing up, we may therefore say that, even with existing methods of research, important advances in our knowledge of the sun could be attained by providing for observations (1) of the solar heat radiation from some high elevation, and (2) of the phenomena of the sun's surface from a site such as Mount Wilson. In the beginning the work should be divided between two sites, but it is probable that the higher station could be given up after the relation of the atmospheric absorption at the two stations should become known. During the same period it would also be necessary to provide for simultaneous observations from a third point many thousands of feet below the high station, to measure the absorptive effect of a known atmospheric layer, in order that the total atmospheric absorption may be determined and eliminated.

New Types of Reflecting Telescopes and Their Use in Conjunction with Laboratory Instruments.

The exceptional opportunity which exists at the present time for advancing our knowledge of the sun by no means depends solely, however, upon the possibility of eliminating a large part of the disturbances due to our atmosphere. Even greater possibilities for

advancement lie in the application of new instruments and methods. Of first importance is the development of the telescope, including: (1) its construction in the horizontal form, especially for work requiring a large solar image and the use of spectroscopes and other instruments from the physical laboratory; and (2) its construction as a large, short focus reflector, equatorially mounted in the *coudé* form, and particularly adapted for the photography of nebulæ, the investigation of stellar spectra, and the study of the heat radiation of the stars.

Astronomical telescopes are of two kinds—refractors and reflectors. The former consist essentially of a lens mounted at the upper end of a tube, which is pointed toward the object to be observed. The lens forms, at the lower end of the tube, an image of the object the size of which varies directly with the length of the tube. Reflecting telescopes, on the other hand, consist of a concave mirror, usually of silvered glass, supported at the lower end of a tube which is open at its upper end. The rays from the object fall upon the mirror, which reflects them back and forms an image at the upper end of the tube. By means of an additional mirror this image is reflected out at one side of the tube, where it may be observed. In both types of telescopes the tubes are pointed directly at the object under observation, and the apparent motion of the object through the heavens is counteracted by a uniform motion of the telescope, produced by clock work.

The development of reflecting telescopes during the first half of the nineteenth century culminated in the great instrument of Lord Rosse, erected in 1845. The crudeness of the mounting of this telescope, due to the lack of suitable engineering facilities, rendered it useless, except for such visual observations as could be made in the absence of a driving clock. Partly for this reason the immense advantages of mirrors over lenses were not discovered, and during the latter part of the century attention was concentrated in large measure on the development of refracting telescopes. These advanced rapidly in size, from the 10-inch telescopes of Fraunhofer at the beginning of the century to the 15-inch Harvard telescope (1847), the 36-inch Lick telescope (1888), and, finally (1897), the 40-inch telescope of the Yerkes Observatory.

(1) But as equatorial telescopes increase in size, it becomes more and more evident that a limit must be set to development in this direction. The driving clock of the Yerkes telescope must move a mass weighing twenty tons with such precision that the image of

the sun will remain fixed in the field of view for hours together. Attachments weighing as much as 700 pounds may be carried at the lower end of the tube, but it is out of the question, in spite of the great size and strength of this telescope, to carry the large spectroscopes of modern times. Through the work of Rowland, whose construction and use of concave and plane gratings has done more than any other one thing to revolutionize spectroscopy, the spectroscope of the physical laboratory has become an instrument of large proportions and of correspondingly great power. Such an instrument may be as much as 40 feet in length, and even if it could be attached to a telescope, the bending of the spectroscope, resulting from its constant changes of position, would render it impossible to obtain sharply defined photographs of spectra. The changes of temperature which occur from hour to hour in an open dome, subject to the fluctuations of the outer air, would also interfere with the use of such spectroscopes for photographic work, even if their rigidity were perfect. As a consequence, the solar spectroscopes in use today are in almost every case practically identical with the instruments of a quarter of a century ago. Moreover, many physical instruments of recent invention and of extraordinary power are so constituted that they cannot be attached to a moving instrument: they must stand absolutely at rest, protected from the most minute disturbances, on massive piers, in a constant temperature laboratory. It is evident, therefore, that a telescope for physical work, if it is to be suitable for use with such instruments, must be so constructed as to bring an image of the sun or of a star into a physical laboratory provided with all appliances necessary to protect the delicate instruments from vibrations, from relative flexure of their parts, or, sometimes, from temperature changes of even a few hundredths of a degree, and to meet other requirements demanded in the most refined research.

The lack of a suitable horizontal telescope made it necessary for Rowland to confine his spectroscopic observations to the light of the sun as a whole, though the use of his powerful spectroscope for a study of the solar details would have yielded results of the greatest importance.

Other requirements of solar work remain to be mentioned. It has been found that a solar image seven inches in diameter offers decided advantages over a two inch image. There can be little doubt that with such atmospheric conditions as exist on Mount Wilson solar images of two feet or possibly even three feet in diameter

could be advantageously employed at times. The use of such large solar images should immediately permit advances of importance to be made. But it would be wholly impossible to secure such images with a refracting telescope of the ordinary type, for the corresponding lengths of the telescope tube would be 215 feet or 322.5 feet, requiring mountings of enormous dimensions, not only excessively costly, but wholly beyond the possibilities of construction. For this reason it is evident that another type of telescope must be employed if such large focal images are to be used.

Fortunately it is possible to meet all the demands imposed by the above named conditions. It is only necessary to mount the telescope tube horizontally in a north and south direction and reflect the light of the sun into the tube by means of a plane mirror driven by clock-work. This mirror is the only moving part of the entire mechanism. Its small size, as compared with a moving tube hundreds of feet in length, not only renders the problem of following the object a simple one, but it eliminates at once the entire question of the enormous cost of the moving tube, the dome, and the great elevating floor which, with an equatorial telescope, would be necessary in order to permit the observer to reach the lower end of the tube in all its various positions.

Heliostats of various kinds have been used for many years, but until recently no serious attempt has been made to construct a large horizontal telescope. The instrument of this type built for the Paris exposition was never completed, and its design was such that the conditions demanded in solar work could not have been met. The recently completed Snow horizontal telescope of the Yerkes Observatory and the horizontal telescope of the Smithsonian Astrophysical Observatory represent the type of telescopes here recommended for solar research. The Smithsonian instrument was designed for bolometric work, and it has proved to be admirably adapted for this purpose. The Snow telescope was designed for solar investigations requiring a very sharply defined solar image, and special precautions were taken to secure this result. Such a telescope accomplishes, at very small expense, the purposes already named; it brings a fixed image of the sun into a laboratory, where it may be observed with large spectroscopes or other apparatus mounted on piers.

These instruments demonstrate the possibility of constructing a much larger telescope of the same type, designed especially for solar work and provided with large spectroscopes and spectroheliographs. To be successful such an instrument should be mounted at a consid-

erable height above the ground, at a site like Mount Wilson, where the atmospheric conditions during the day are exceptionally fine. Under such circumstances a sharp and well defined solar image, from two to three feet in diameter, could be obtained, and numerous researches now entirely out of reach could be undertaken with every reason to hope for success.

Dr. Elihu Thomson has suggested that any distortion of the mirrors by the sun's heat can be obviated by making them of fused quartz, since the coefficient of expansion of this substance is almost inappreciable. Since Dr. Thomson has already succeeded in making small mirrors in this way, we include an item to cover the expense of the necessary experiments, which Dr. Thomson has very kindly volunteered to superintend.

(2) In work on the sun, as already remarked, the most important requirement is a large solar image produced by a telescope of great focal length, but in order to trace out the successive stages in the development of stars like the sun, a telescope of a very different type is required. The remarkable opportunities for advance in astronomy which exist at the present time through the possibility of building a large reflecting telescope were outlined in the Year Book of the Carnegie Institution for 1902 (p. 141). The optical parts of the Snow horizontal telescope consist exclusively of mirrors, and it thus preserves the peculiar advantages of the reflecting telescope; but for the photography of faint nebulæ, and for many other similar researches of fundamental importance in the study of the sun's origin and development, this type of telescope is not well adapted. What is needed is a mirror of the largest possible diameter and of short focal length, provided with a heavy and well constructed equatorial mounting.

To give an idea of the immense advantages of an instrument of this kind it may be recalled that with a two foot reflector an exposure of 40 minutes suffices to photograph stars that are invisible in the largest refracting telescopes. With longer exposures millions of stars can be photographed with such a reflector, of whose existence the largest refractors could never give any indication. A 5-foot reflecting telescope would collect six times as much light as a 2-foot, and nearly three times as much as the largest reflector now in use, and would open up certain fields of investigation now entirely closed. It would furnish means of photographing the nebulæ which would probably be superior to those offered by any existing telescope. It would also permit the heat radiation of some of the brighter stars

to be measured, from which valuable conclusions could be drawn as to their physical nature, and it would furnish the essential means of studying stellar spectra on a larger scale than that afforded by existing instruments.

Up to the present time such powerful grating spectroscopes as those of Rowland have not been employed for the study of stellar spectra. As already stated, the spectroscopes are too large to be adapted to equatorial telescopes, and the feeble light of the stars would demand exposures far longer than can be given under present circumstances; but with a great reflecting telescope, so mounted as to produce the image of a star in a constant temperature laboratory, there should be no serious difficulty in photographing the spectra of the brightest stars with the most powerful grating spectroscopes. The exposures might have to be prolonged for several nights in succession, but it would only be necessary during this time to maintain the spectroscope rigidly mounted on fixed piers at a constant temperature. With such photographs of stellar spectra a large number of problems, of great importance in connection with questions of solar physics, could be solved. For example, it would be possible to determine beyond question whether, as is now believed by some investigators, the red stars have on their surface a great number of spots like those of the sun. If the presence of numerous sun spots on these stars could be proved it would follow that as the sun grows colder, and advances toward the condition of the red stars, the spots on its surface will multiply in number. Such a conclusion would have a very important bearing on the problem of the solar constitution. Many other similar questions could be answered, such as those which relate to the relative pressure in solar and stellar atmospheres, the changes in the solar spectrum which will result from decreasing temperature, etc.

General Nature of the Principal Problems of Solar Research.

It should be evident from what has been said that improved instruments and methods of research, employed under atmospheric conditions more favorable than those experienced at existing observatories, should render important advances possible. Let us now consider briefly some of the principal problems of a solar observatory.

The Constitution of the Sun.

The problem of the solar constitution, though repeatedly attacked on both observational and theoretical grounds, still remains unsolved.

In addition to the bolometer, referred to elsewhere, the two principal instruments employed for this work in conjunction with the telescope are the spectroscope and the spectroheliograph. The former permits the investigation of the nature of the chemical elements in the sun, their physical condition, and their motions in the direction of the earth, while photographs taken with the latter show the distribution of the various elements in the solar atmosphere and on the sun's disk. The two instruments supplement each other most effectively, and can be used to great advantage in connection with the bolometer and other physical apparatus.

The sun closely resembles the earth in chemical composition, but differs from it in every other particular. Intense heat, indefinitely greater than that of a Bessemer converter, maintains its substance in a state of vapor. The visible surface of the sun marks the limit where the metallic vapors of the interior, coming into contact with the cold of space, condense into luminous clouds. This surface presents a granular appearance, since the bright clouds form the upper extremities of columns of vapor ascending from the interior, separated by spaces filled with cooler and less luminous vapors. Above the visible clouds columns of uncondensed vapors continue to ascend. Hydrogen, helium, and calcium rise to heights of several thousand miles, and at certain points project from the nearly continuous sea of flame (the chromosphere) in the form of great gaseous prominences, ranging in altitude from 15,000 to 300,000 miles. At times of greatest solar activity violent eruptions frequently occur, producing prominences which sometimes rise to a height of nearly 300,000 miles in less than half an hour.

Formerly the flames of the chromosphere and prominences were visible only at the sun's circumference, when at times of total eclipse the dark body of the moon intervened to cut off the overpowering illumination of the earth's atmosphere. In 1868 it was found that they could be observed in full sunlight with the spectroscope, and in 1892 they were first successfully photographed with the spectroheliograph. This instrument also permits the flames to be photographed in projection against the sun's disk, thus rendering possible the investigation of a great variety of new and remarkable phenomena. The invisible vapors of calcium, and recently, through the application of the Rumford spectroheliograph of the Yerkes Observatory, those of hydrogen, iron, magnesium, or any other substance present in the chromosphere, can be photographed at will, and their forms, distribution, and motions investigated.

The peculiar differences in the behavior of the various elements and the characteristic parts they play in solar storms suggest questions which are quite as important to the chemist or the physicist as to the astronomer. The high temperature and the enormous masses of material involved in these solar phenomena far surpass the possibilities of laboratory experiments. If the elements can be broken up into simpler forms by intense heat, as the tendency of modern research seems to indicate, the best chance of detecting evidence of such dissociation would appear to be in the sun. If calcium, for example, can be separated by heat into several constituents, these can be photographed with the spectroheliograph in solar storms, where their difference in behavior should betray their separate existence. Results already obtained at the Yerkes Observatory point to interesting possibilities in this direction; but, in order to deal with the problem successfully, apparatus much more powerful than that now employed must be available for use under superior atmospheric conditions.

Problems without number relating to the solar constitution are ready for solution and demand only a carefully planned attack under suitable conditions. The peculiarities of the solar rotation, rapid at the equator and decreasing toward the poles, have been but little studied. The distribution of the elements in the lower chromosphere and the explanation of the absorption which produces the dark lines of the solar spectrum have been subjects of dispute for many years. This problem is now studied only at eclipses, but a large solar image, observed on any clear day with a powerful spectroscope in a good atmosphere, would permit important advances to be made. The true cause of the darkness of sun spots is not yet understood, and it is even uncertain whether they are cavities or elevated regions. Their minute structure and the remarkable phenomena of their chemical composition will afford indefinite opportunity for research.

All of these questions and many others are closely dependent upon the sun spot period. As the spots increase in number and activity all other solar phenomena vary in sympathy with them, increasing toward the period of greatest intensity and then fading away toward the time of calm. The investigation should therefore extend over a term of at least eleven years, and preferably until after the maximum which occurs about 1916. The advisability of continuing the work longer may be left to be determined in the light of the results obtained.

The Heat Radiation of the Sun.

The advantages of studying the heat radiation of the sun at high altitudes are well illustrated by the results obtained by Langley from the summit of Mount Whitney in 1881. The dryness and purity of the air at this elevation in the Sierra Nevada are perhaps unsurpassed at any other mountain station. Over one half of the atmosphere lies below the summit of the peak, and this comprises the denser and more variable strata, which interfere most with determinations of solar radiation. No sooner had Langley's delicate apparatus been set at work than a new class of solar rays previously unknown was discovered. Previous estimates of the absorptive effect of the lower regions of the atmosphere were found to be far below the truth, and the value of the solar constant, which measures the amount of heat received by the earth from the sun, was immediately increased by about one half. The new radiations which were found to possess so great importance cannot pass through glass, and prisms of rock salt must be employed in studying them.

The Mount Whitney observations extended over only a few weeks, and were made with apparatus which, from his present point of view, Professor Langley would consider extremely imperfect. In spite of the unfavorable conditions that prevail at Washington, Langley has continued his investigations at the Smithsonian Institution and developed his methods to an extraordinary degree of perfection. Slow and inaccurate observations with insensitive instruments have given place to automatic records of the highest precision, secured by the aid of photography with instruments so sensitive that differences of temperature of less than one ten-millionth of a degree centigrade can be detected.

The problems which should be attacked from the summit of Mount Whitney, or some equally good station, with the refined instruments now available, cover a wide range. The most important single question concerns the intensity of the solar radiation. Is this constant, or does it vary during that well defined period of about eleven years in which solar phenomena are known to pass from a state of comparative calm to one of violent activity, and then again to subside to their former condition? At times of sun spot minimum the sun's surface for months together is wholly devoid of spots. Faculæ and prominences are few and inconspicuous, and the spectroscope shows little, if any, evidence of disturbances of any kind. Gradually, however, spots begin to appear, and then rapidly multiply in number.

The sea of flame which surrounds the sun increases in brilliancy and activity. As the spots continue to increase in number and area, eruptive phenomena on a tremendous scale become more and more frequent. At the time of maximum activity the violence of these disturbances and the rapidity with which flames hundreds of thousands of miles in height form and disappear surpass all comprehension. In view of these facts it is not surprising that the question has been raised whether the total radiation of the sun does not undergo variations corresponding in some measure with these variations in the violence of the phenomena visible on its surface.

The solution of this problem, as has already been pointed out, requires that observations be made under conditions not now available. With instruments of the modern type installed at a great altitude and with similar instruments at a second station some thousands of feet below, the principal conditions needed for the solution of this question would be provided; for it is necessary not only to measure the intensity of the sun's heat from an elevation so great as to eliminate most of the obstacles interposed by the denser and more fluctuating portions of the earth's atmosphere, but also to arrive at a more thorough understanding of the absorption which solar rays undergo in passing through our atmosphere, and particularly to determine the difference in the quality and degree of this absorption at different levels. The study of the earth's atmosphere is of great importance in connection with this research. Indeed, it is not improbable that after the completion of a thorough investigation, carried on simultaneously at the upper and lower stations and extending over a sufficient period of time, it would be possible to accomplish all of the purposes of a solar observatory at a lower station.

When observed in the telescope the sun's disk is found to be much more brilliant at the center than near the circumference The difference is so great that it was detected in the earliest observations of the sun, made with the imperfect instruments of the time of Galileo. This difference is due to an absorbing atmosphere which completely surrounds the sun, and reduces the intensity of the light and heat radiated outward through it. In a study of this absorption made by Vogel in 1877 it was found that at the edge of the sun's disk only about 13 per cent of the violet rays escape. The percentage of transmitted light increases progressively for the blue, green, and yellow rays, until it amounts to 30 per cent for the red. From these results it has been concluded that if the absorbing atmosphere were

removed the intensity of the sun's light would increase by as much as 1½ times for the red rays to 2½ times for the violet rays. Under such circumstances the color of the sun would appear blue. Later results indicate that the removal of the sun's atmosphere would increase its radiation of heat about 1.7 times. It is evident that if there were any considerable variation in the absorptive power of the sun's atmosphere, due to such changes as might easily take place during the passage from minimum to maximum solar activity, the total radiation of solar heat might be very appreciably increased or diminished. Indeed, such a variation in the solar absorption is considered by Halm, in his recent memoir on the solar constitution, to be the cause of the eleven year sun spot period. It is therefore evident that the plan of observations should include an investigation of the heat radiation of various parts of the sun's disk, carried on during a term of years.

Evolution of the Sun and Stars.

We have already called attention to the fact that while telescopic observations may give us an intimate acquaintance with the sun's surface phenomena, from which conclusions may be drawn as to its physical constitution, they can give us no direct information as to the past or the future of the sun. We must seek such information in the stars and nebulæ. Keeler's work with the Crossley reflector of the Lick Observatory brought out the extraordinary fact that there are in the heavens at least 100,000 nebulæ, the majority of which are of spiral form. It is the belief of many astronomers that this spiral form is indicative of a process of development, and that the teachings of the nebular hypothesis must be modified so as to accord with this new point of view. Advantage should be taken of every possible increase in the scale of the photographs by which the forms of spiral nebulæ are recorded, in order that further data may be obtained that may be of service in future attempts to modify the nebular hypothesis. Photographs taken with all possible instrumental refinements might well be expected to show changes in the nebulæ after the lapse of a comparatively short period of time.

While we may thus seek in the nebulæ for evidences of the sun's origin, its early life and subsequent development may be traced in the stars; for, in spite of their enormous distance from the earth and the consequent impossibility of observing their surface phenomena as we do those of the sun, the spectroscope is competent to

give reliable information as to their physical condition and state of development. The secular changes of any star are so slow that thousands of years might be required to detect them. But it fortunately happens that all stages of growth are now represented. It is as though one were to pass through an oak forest and seek to learn of the development of the trees. During the period of his stay no apparent change would occur. But he would have before him trees of the same species in every stage of growth: the acorn, the sapling, the oak in its prime, and the dying tree, representing the last stage in the evolutional process. By observing these, the evolution of a single tree could be understood. Thus in the stars, while their changes are too slow to be observed, a similar opportunity exists of tracing their life history. Preceding the condition of the sun we find stars like many of those in the constellation Orion, which have advanced but little beyond the state of nebulæ. Next to these come white stars like Sirius, which are but slightly condensed, and represent what the sun must have been millions of years ago. From these it is possible to pass by gradual steps through the process of development which leads to the production of yellow stars like the sun. Here the process of decline has apparently set in, and the beginnings of the last stage are faintly visible. Finally come the orange and red stars, the spectra of which reveal phenomena of the greatest interest, such as the sun will exhibit after the lapse of many centuries.

Existing instruments have sufficed to develop the main lines in this process of evolution, but there are numberless questions which cannot be answered without the aid of much more powerful telescopes. A great reflector, such as would be most suitable for photographing the nebulæ, would also be much superior to any existing telescope for the spectroscopic investigations which furnish the data required in this research. Reference has already been made to the possible existence of great sun spots on the red stars; these and many similar questions of equal importance in connection with the constitution of the sun cannot be solved until such instrumental means have been provided.

Throughout all of this work, both on the sun and the stars, it would be necessary to have constant recourse to laboratory experiments in order to interpret the observed phenomena. A suitable equipment of physical apparatus for this purpose is therefore essential in connection with a solar observatory.

Plans and Estimate of Cost.

As already explained, a principal station and two temporary auxiliary stations will be needed for the proposed Solar Observatory. We recommend that the principal station (A) be established at some such point as Mount Wilson, where Professor Hussey's observations have shown that the conditions are excellent for work on the sun requiring very perfect definition of the image. For the study of the solar constant and the absorption of the earth's atmosphere two auxiliary stations (B and C) will be required for use during the summer months. Station B should occupy a site at the summit of a high mountain, while station C should be near the base of the mountain, some thousands of feet below. In view of the time and expense required, Professor Hussey's study of mountain sites did not include the very high mountains. We have obtained much valuable information regarding conditions on Mount Whitney and other mountains, but a further study of the matter would be required before the sites for stations B and C could be decided upon. While Mount Wilson would probably serve the purposes of the principal station, there should nevertheless be a further and more prolonged study of sites for this purpose, to be made while the equipment is under construction. Recent experience indicates a probability that stations B and C might be discontinued after observations have been made at these points for two or three seasons, since it is probable that all the work of the observatory could then be done at such a point as Mount Wilson.

Principal Station (A).

The summit of Mount Wilson is about 8 miles in an air line from Pasadena. At present it is reached from the base of the mountain by either one of two trails, suitable for pack animals, but not adapted for wagons. One of the first considerations, therefore, would be the problem of providing adequate transportation facilities. This might be done (1) by constructing a wagon road from the foot of the mountain to the top; (2) by constructing an electric railway from Pasadena to the summit of the mountain; (3) by extending to Mount Wilson the electric railway that now terminates on Mount Lowe. Owners of the property on the summit of Mount Wilson state very positively their conviction that one of the two latter projects would be carried out in case this site were selected. This would naturally be an important consideration in the final decision

upon the question of site. Estimates of the cost of construction are based on the assumption that such means of access will be available.

Plan of Work.

The general plan of work which we believe should be undertaken at this observatory would comprise the following classes of observations. This program is of course subject to such modifications as experience may suggest.

(1) Frequent measurement of the solar constant, together with studies on the absorption of the solar atmosphere and the radiation of different portions of the sun's image, such as spots, faculæ, and prominences. The principal instruments needed for this research are a 16-inch cœlostat and large spectro-bolometer for the solar constant work, and a 30-inch cœlostat, with concave mirror of about 200 feet focal length, for providing a solar image suitable for detailed radiation work.

(2) Systematic observations, with large spectroscopes and spectroheliographs, on such problems as the solar rotation, the structure and nature of sun spots, faculæ, etc., and other problems related to the solar constitution. For this work there will be required two 30-inch cœlostats, used in conjunction with objectives and mirrors ranging in focal length from 64 feet to 200 feet; two large plane grating spectroscopes, having focal lengths of about 21 feet and 42 feet respectively, provided with auxiliary apparatus for work with the spark and arc; a three-prism spectroheliograph of about 10 inches aperture, and a three-prism spectroheliograph of 8 inches aperture and about 33 feet focal length. In order to secure the best definition of the solar image, the cœlostats should be mounted at a considerable height above the ground, and electric fans should be provided for stirring the air by Langley's method.

(3) Astrophysical researches on stars and nebulæ with a large reflecting telescope, provided with a three-prism spectrograph, and also with a concave grating spectrograph mounted in a constant temperature laboratory, for use with the reflector arranged as an equatorial *coudé* in photographing stellar spectra with very high dispersion.

(4) Laboratory investigations, mainly of a spectroscopic nature, on problems arising in connection with the solar and stellar work. Much of the apparatus provided for this purpose should be mounted and used in connection with the solar spectroscopes.

Buildings.

The buildings for the observatory should be of the simplest construction, designed for results, rather than for appearances.

A. Spectroscopic and Bolometric Laboratory.—Experience has shown that in order to obtain good definition during the day the instruments should be mounted at the greatest practicable height above the ground. In the preliminary design for a separate spectroscopic laboratory, the cœlostat piers were accordingly carried up to a height of 40 feet. A sketch was also made for a separate bolometric laboratory. Subsequently, from motives of economy, a combination was made of these two laboratories, wherein provision is made for one 16-inch and two 30-inch cœlostats. The two large cœlostats would be used for work requiring an image of the sun—one of them for spectrobolometric observations, the other for work with the solar spectroscope and spectroheliograph. The 16-inch cœlostat would be employed for measurement of the solar constant. The spectroscopes and spectroheliographs are mounted on piers so high as to be above the level of the constant temperature house for bolometric work. The piers may be constructed either of steel, properly ballasted, or of granite, which is found in abundance near the summit of Mount Wilson. The laboratory itself is of wood, supported on steel construction, anchored to the rock. In the plans submitted provision is made for mirrors of 200 feet focal length, but it is probable that it would ultimately be desirable to extend the house 100 feet to the north, thus permitting the use of a mirror of 300 feet focal length. The construction of the building is such as to reduce to a minimum the heating of the walls and the consequent currents of warm air.

B. Reflector Dome.—For a reflecting telescope of 5-feet aperture, mounted in accordance with the general plan shown in the drawing,* a dome of 50 feet internal diameter is required. The walls of the tower are built of thin she et iron, the track for the dome being supported upon columns of steel construction. The sheet iron wall is covered on the outer surface with wooden louvers in order to prevent heating by the sun. South of the telescope pier is a constant temperature laboratory, with piers for the concave grating spectroscope.

C. Office Building.—This includes offices and computing rooms for all the members of the staff, together with shops for instrument construction, library, laboratories, photographic rooms, etc., suitably

* Not here reproduced.

equipped for work. This building might be constructed of wood or possibly of rough fragments of granite. In any event, provision should be made for storing the photographs and other records in a small fire proof room.

In view of the isolation of the observatory and the important purpose of developing new methods and apparatus in connection with its work, provision has been made in the estimates for a very complete equipment of the instrument shop. It is understood that this machinery would be purchased and immediately installed in a shop if the Carnegie Institution were to decide to establish the observatory. The tools would therefore be used from the outset in the construction of a large part of the equipment.

D. Dwellings for Members of Staff, etc.—Simple cottages and bachelors' quarters should be provided at the observatory site.

Other items of expense of a general nature would include a telephone line and a line for transmitting electric power to the summit of the mountain, sewer and water systems, etc.

Stations B and C, for Solar Observations at High Altitudes and Studies of Atmospheric Absorption.

No recommendation is made at present as to the site of these stations, although it is possible that Mount Whitney and Lone Pine would prove to be suitable. The estimates of expense are based on the assumption that sites similar to these would be selected.

The plan of work at these stations would include simultaneous observations for the measurement of the solar constant and the determination of the atmospheric absorption. The buildings of the two stations should be alike, except for the addition of a few rooms required at the lower station. They contain, in addition to shelter for the instruments, small dwelling rooms for the four men—two at each station—who would occupy them throughout the summer months. The instrumental equipment of the two stations would be similar, consisting of a 16-inch cœlostat and spectro-bolometer for the solar constant work, together with a small miscellaneous equipment of instruments required in connection with this investigation.

General items of expense in connection with stations B and C would include means of communication between the stations, simple devices to maintain the constancy of temperature needed for bolometric work, provision for water supply and fuel, improvement of trails, shelter for laborers and pack animals, etc.

Staff.

A director, in general charge of all work, with special duties in connection with solar spectroscopy.

At Principal Station (A).—

Bolometry: An observer in charge of all bolometric work, with special duties in connection with the spectro-bolometer; an assistant observer at the spectro-bolometer, and four computers.

Solar spectroscopy: Two assistant observers, to carry on, with the director, solar spectroscopic work; two computers at the beginning, with the probability that this number will have to be increased during periods of great solar activity.

Stellar spectroscopy: One associate and one assistant observer; one computer, and one night assistant, on duty with the large reflector.

In addition, there would be required a secretary-librarian, who would also have charge of the accounts and the purchasing of supplies; a stenographer, an instrument-maker, a skilled machinist, a carpenter, a janitor, and two laborers.

At Station B.—One observer, one assistant observer, and a man-of-all-work during the summer months.

At Station C.—A staff like that at Station B, with the addition of one instrument maker.

LEWIS BOSS, *Chairman.*
W. W. CAMPBELL.
GEORGE E. HALE.

OCTOBER 8, 1903.

APPENDIX A TO REPORT OF COMMITTEE ON OBSERVATORIES

REPORT BY W. J. HUSSEY ON CERTAIN POSSIBLE SITES FOR ASTRONOMICAL WORK IN CALIFORNIA AND ARIZONA

Prof. LEWIS BOSS,

Chairman Astronomical Committee, Carnegie Institution.

SIR: Acting under your instructions, I left San Jose for southern California on April 16, 1903, to examine into conditions for astronomical work in that section, especially with reference to research upon the sun. I was to bear in mind the following requirements: First of all, excellent day seeing; scarcely second to this, excellent night seeing; fitness of site for living conditions, accessibility, availability of power, electric or other, either from abundant water sources or from commercial distribution, etc.

For the purposes of this survey, a 9-inch achromatic objective, focus 108 inches, was kindly loaned to the Committee by the Alvan Clark and Sons Corporation. The Lick Observatory loaned a Warner and Swasey micrometer, a declination axis and slow motion by the same makers, a good centrifugal driving clock, and many smaller pieces of apparatus needed in this work. A prism-and-grating spectroscope was loaned by the Chabot Observatory, and an excellent helioscopic eyepiece was supplied by the Yerkes Observatory.

The mounting of the telescope was made in the Lick Observatory instrument shops from my designs. Its general features will be understood from the accompanying photographs.* It was expected that tests in out of the way places would be required, and on this account the mounting was made as light as was consistent with sufficient rigidity, and the parts were so arranged that they could be transported on pack animals over mountain trails. The tube is constructed of aluminum sheets one sixteenth of an inch thick, suitably strengthened by end and center castings, etc. Its weight, complete with lens, tail piece, micrometer, and counterweights, is only 88 pounds. The wooden polar axis is provided with steel trunnions which turn in roller bearings.

* Not here reproduced.

The object glass is of excellent quality, and the mounting met the requirements in a satisfactory manner.

The equipment had been shipped ahead and was awaiting me in Los Angeles. In accordance with your instructions, I at once arranged for its transfer to Echo mountain, where Professor Larkin very kindly put the resources of the Lowe Observatory at my disposal, in order that comparative tests might be made with the Lowe 16-inch telescope and my instrument.

President H. E. Huntington, of the Pacific Electric Railway Company, through his general manager, Mr. Epes Randolph, extended the courtesies of the Mount Lowe Railroad from Los Angeles during my stay, thus facilitating the work.

Mount Lowe, like its neighbors of the San Gabriel range, rises most abruptly from the Los Angeles plains. One approaches the mountain by a mesa which rises so gradually to the precipitous spurs that he scarcely notices he has left sea level behind. Then a cable incline lifts him suddenly to an altitude of 3,200 feet. At the head of the incline are the power house and other buildings of the electric railroad. Some abandoned chalets stand by the edge of the canyon, whose precipitous eastern walls send back the echoes that give the place its name. Properly, Echo mountain is but a spur of Mount Lowe, which is a vast pile of just such spurs, culminating in a round, rocky, desolate summit, at 5,650 feet above the sea.

The original intention, it is said, was to place the Lowe Observatory on the top of Mount Lowe, but the electric railroad was never completed to the summit, and this plan was not carried out. The present terminus of the road is at Alpine Tavern, 4½ miles from the top of the incline and at 1,100 feet greater elevation.

The Lowe Observatory is situated at Echo mountain, 3,400 feet above the sea, a short walk above the head of the incline, and by the electric road is within fifty minutes of Pasadena and an hour and twenty minutes of Los Angeles.

The mountain rises immediately back of the Observatory, on the north, to an angular altitude of perhaps ten or more degrees, so that there is not a clear horizon in this direction. Toward the northeast, across a canyon, another spur rises to a greater elevation, and here, too, the view is obstructed. In other directions the horizon is clear.

The water supply of the Observatory is derived from a spring in one of the canyons a short distance away. A reservoir has been

constructed just in front of the Observatory and a few feet below. The water from the spring is piped into this, but not into the Observatory, the elevation of the spring being insufficient for this purpose.

Judging by surface indications, Mount Lowe as a whole is not well supplied with water. Nevertheless, in its canyons, even well up toward the top, one may find clumps of bay and sycamore trees, and these are known to require a fairly constant supply of water. It is possible, therefore, that by judiciously tunneling in the vicinity of these trees water could be developed in places where none is now visible on the surface except immediately after rains.

It soon became apparent that the prevailing level of the fog in the Los Angeles region is higher than that for the corresponding season in the neighborhood of San Francisco. It also happened to be the season of maximum drift, namely, April and May. I was therefore detained at Echo mountain beyond the time limit first set for my stay, the fog being around or above me fully two thirds of the time.

For this and other reasons, the suggestion to make tests at two other stations on Mount Lowe was modified, and only one was chosen. This was Inspiration point, at about 4,500 feet altitude, and accessible from Alpine Tavern. Here fog did not trouble, and tests were soon completed, several runs being made by the electric road down to Lowe Observatory for comparative tests with the 16-inch. On the 13th of May the 9-inch was finally dismounted, and the equipment was packed and sent to Los Angeles for shipment to San Diego.

My stay at Mount Lowe had shown that an elevation of 2,500 feet would in this section be entirely insufficient to escape the prevailing height of either the fog or the dust. It had also impressed upon me the enormous advantage of quick and ready communication with prosperous communities, such as the electric road made possible with Pasadena and Los Angeles, and of the electric power which that brought to hand for mechanical work.

I have spoken of the abruptness of the San Gabriel range. The usual mountain masses, however lofty, have a low altitude compared with the measure of their bases. Perhaps an average proportion in the California coast ranges would be that a mountain with an altitude of 1 mile would run a base line 15 miles to reach the last of its foothills. Mount Lowe has no foothills, and falls to the mesa's edge in 2½ miles. Mount Wilson, adjacent on the east, higher and broader, holds all its southern spurs well within a sweep of 4 miles

from the summit. Their slopes are practically unscalable except by constructed trails. Where the steepness does not forbid the way, the chaparral everywhere disputes it. This brushwood growth is peculiarly characteristic of the semi-arid mountains of southern California. Mount Lowe wears this covering of chaparral, but it is nowhere luxuriant. The decomposing white granite is inhospitable soil for vegetable growth. The mountain is sterile and forbidding, except in a few of its canyons. The white granite sand, scarcely covered at all with accumulating humus, stares everywhere through the irregular lanes in the chaparral. In many places the faces of the slopes lie almost bare and glitter unpleasantly in the sun. There is little doubt that radiation from the exposed surfaces accounts for the fact that the day seeing both at Echo mountain and at the higher station at Inspiration point averaged lower than the night seeing, which was, on the whole, excellent.

The term mesa is used in the Southwest to denote the fringe of detritus washed down from the mountains and sloping away from their bases to the plains, or the mesa may be formed, as in the region about Flagstaff, Arizona, by lava flows from volcanic cones; but always, if one examines it as a whole, one finds a sloping upland contiguous on the upper side to mountains and on the lower side to plains or valleys. It is in this sense of contiguous sloping uplands that I use the word in this report.

My departure from Los Angeles for the south was somewhat delayed by the accidental side-tracking of a part of the freight in its shipment down from Echo mountain. In the meantime I made my first trip to Mount Wilson, going up the Sierra Madre trail on the east side, staying over night, and descending the west or Pasadena trail on the second afternoon. Some days previously I had the good fortune to make the acquaintance of Mr. T. P. Lukens, of the United States Forestry Commission. He has been of the greatest assistance to me in furnishing reliable information respecting the mountains of southern California and northern Arizona. He spoke enthusiastically of Mount Wilson and advised me to see it. The only access to this peak at present is by the trails just mentioned. These are only wide enough for pack animals, burros, or mules, and about four hours is required for the ascent or descent. I had been assured that Mount Wilson was very different from Mount Lowe, though from the valley it has the same grim outlines; but I was quite unprepared for the actual view of it. Instead of one barren rock, a succession of rolling knolls forms the summit. The canyons con-

tain spruce and the ridges are covered with chaparral—a growth of Ceanothus (buckthorn), scrub oak, and other evergreen bushes, so luxuriant, so dense, that passage through it is quite impossible without the aid of an ax. This appealed to me at once as an ideal covering, changeless the year around, for the checking of solar radiation.

On exploration, Mount Wilson seemed to have in addition these points of advantage: A water supply remarkable for abundance and nearness to the summit; a small peak adjacent to and above the source of this water, somewhat higher than Wilson's peak, which could be utilized for pressure in a water system; abundance of granite rock, both dark and light, some of it showing excellent cleavage, for building purposes.

Absence of wind, as reported by every one acquainted with the place, is evidenced here by the straightness and symmetry of the trees, one apparent exception being the Ponderosa pine, with a great flat crown as if bent under the pressure of storm and wind. Later, I learned from a paragraph in a report of the United States Forestry Commission that the peculiar shape of this tree is a matter of age and habit and not due to local conditions.

The live oak is everywhere, both as tree and shrub, and in the canyons maple, alder, sycamore, bay, and cottonwood are found. Of the conifers, the big-coned spruce most abounds, and it is to this and the chaparral that the mountain owes its unbroken slopes of green.

In one thing only does Mount Lowe have an advantage, and that is in the possession of an electric road. As to electric power for mechanical and other uses, a line 4 miles in length from Wilson peak would connect with existing systems. An electric railroad could be constructed from Pasadena to the summit of Mount Wilson quite as easily as to the summit of Mount Lowe; or, as an alternative, it would be possible to extend the present railway from Alpine Tavern around Markham and San Gabriel peaks to the summit of Mount Wilson. It is estimated that such an extension would not need to be more than 10 miles in length.

On the evening of May 19 I reached San Diego and occupied the following day with business matters and acquiring information concerning conditions in the back country, especially as to the routes into the mountains. On the 21st I made an early start for the nearest of these, San Miguel, a peak 2,600 feet high, standing conspicuous and alone about 13 miles in an air line east of the town. The trip was made in a buckboard, as far as there was any

road, to the foot of the mountain, and then on horseback to the summit. Clouds lowered all the morning and hid the peak during the approach, but I had a clear and excellent view from the summit. I saw a country very different from the Los Angeles plains. It looks like an old land, and has the simple form and coloring that distinguish the arid regions everywhere. Sharp ridges rise ever more numerous back from the sea, but among the rolling dunes occasional peaks stand alone, boulder-strewn and desolate. San Miguel itself is a type of these—treeless from its top to its base.

The horses picked their way without difficulty through the scanty chaparral. Most of it stood just to their knees, though now and then a thicket of buckthorn rose to the riders' shoulders. From the summit to the base I looked in vain for any sign of springs or live water-courses, and there was almost no trace of animal life.

Here and there in the valleys below shone little reservoirs of water made by damming open streams. Now, at the close of the rainy season, none of these were full. I was told that the great Sweetwater dam, built at a cost of a quarter of a million dollars, has scarcely 10 feet of water behind it. There seems to be no bottom to this land, and in a season of ordinary rainfall most of the water sinks out of sight before it gets to these dams. The higher mountains, with their winter snows, are 40 miles away, and fluming is expensive business.

Riding back to town I read everywhere the story of the land of little rain. Just for a few miles at the foot of San Miguel a grassy mesa rolls irregularly down, but passes soon into the dunes of cactus and prickly pear which extend to the sea. In the little winding valleys orchards are dead or dying. Along the dry bed of the Sweetwater river pumps are trying to recover enough to save the trees of that district.

San Diego is an incorporated city of perhaps 18,000 people.

I went to Lakeside a few days later, and the San Miguel experience was in essentials repeated. Occasional orchards thriving, more abandoned, dying, or dead, according as owners have been able or not to hold out through the dry years and pay the high price of water in a drouth. At Lakeside I took the stage for Cuyamaca, a ride of 35½ miles. This region was chosen as the site of my station for the following reasons: My observations in San Diego had led me to believe that in this region, as at Echo mountain, any altitude below 4,000 feet would often be covered by fog at this season. On this account, as well as on account of the apparent lack of water within

1,500 feet of the summit, the establishing of a station on San Miguel seemed inadvisable. Of the higher mountains, Cuyamaca and Palomar seemed to promise most, but in San Diego I was able to get little definite information as to the latter. The impression prevailed that it was very inaccessible. Moreover, Cuyamaca was one of the mountains suggested by your Commission as worthy of exploration, and it has, besides, a daily mail stage from Lakeside to the Stonewall mine at Cuyamaca lake. The latter is at an elevation of about 4,500 feet, and there it seemed most expedient to make my first camp.

In San Diego I had received much valuable assistance from Mr. Ford A. Carpenter, local forecast official of the United States Weather Bureau. Through his interest I became acquainted with Mr. M. C. Healion, president of the San Diego Flume Company. This company controls the water from Cuyamaca lake, and has a house there. President Healion very kindly placed a room at my disposal, and his foreman in charge proved a very helpful assistant.

Cuyamaca is an Indian word, said to mean "cradle of the rains," and here is the heaviest rainfall in southern California. The lake is 2 miles long and less than 1 mile in width. The three Cuyamaca peaks rise about it—South peak to 6,500 feet, and Middle and North peaks to approximately 6,000 feet. Rattlesnake hill and a low circle of chaparral-covered hills shut off the view of the Colorado desert, whose first sentinel peak, just visible from the middle of the lake, is barely 6 miles distant in an air line, for Cuyamaca drops abruptly on the east. This nearness of the desert augured ill, and indeed the pull between the great oven and the sea kept the winds at work. The telescope was erected on the green slope at the base of Middle peak, near the dam and the west shore of the lake.

Grass is abundant here. Forests of oak rise back of the meadows, and above them the fir, the cedar, and the pine, in turn. The summits are forbidding, and their ascent with horses is impracticable unless road or trail has been cut. Up South peak such a road was made a few years ago by the United States Geological Survey, and that ascent is now easy. The view from this summit, supplemented by constant reference to the topographical map prepared by the United States Geological Survey, gave me an excellent idea of the entire surrounding country. To the north lay Palomar, beyond which rose the white cap of Old Baldy in the San Gabriel range, 110 miles away. Thence the San Bernardino and San Jacinto mountains lead toward Cuyamaca and the connecting ranges that

drop to the great desert on their eastern slopes. The Laguna mountains reach up from Mexico and thrust out a shoulder that just intercepts the view of the great Colorado floor stretching east to Yuma. Down to the west and south fall the lesser hills to the sand dunes and the sea, a country all creased and crumpled, arid and brown. Just below Cuyamaca lie Viejas and Elcajon, peaks whose altitudes and situations had appealed to me on the map as promising sites to consider. The field glasses bring them within a mile or two, and I find them, as I feared, exaggerated San Miguels; Elcajon more precipitous, Viejas more barren, equally treeless, and by all reports equally destitute of springs above their bases.

I learned that frosts may occur any month in the year at the lake; that the thermometer may rise above 100° in summer and fall below zero in winter; that the winds blow very nearly all the time, and that the lake dries up in summer.

The seeing at Cuyamaca proved to be what one might expect in a region of high winds and rapidly changing temperatures, and all that could be learned by inquiry and inspection failed to indicate anything more promising in this extreme southern end of the state, excepting Palomar only.

I had been told that one must return to San Diego, go by rail to Escondido, and thence by stage in order to reach Palomar. But I had looked across to it repeatedly from both North and South Cuyamaca peaks, and had concluded that one should be able to reach it on horseback from Cuyamaca, through Julian and either the Santa Ysabel or San Felipe valley into Warner's Ranch valley, which is at the southeastern base of Palomar. I set out, therefore, on the morning of the 6th of June. As I rounded the lake and turned northward I encountered light breezes from the desert. The peculiar subtraction of vital force effected by these desert winds is scarcely to be understood, but it is never to be denied by those who have felt it.

After a hard, all day's ride of about 40 miles via Santa Ysabel valley, through canyons, over hills, and across valleys, traversing roads at times remarkable for steepness, I reached Cook's ranch, on a shoulder of Palomar, late in the evening. I made excursions over the mountain on the following day, and the next day returned to Cuyamaca.

Nothing prepares one for the surprise of Palomar. There it stands, a hanging garden above the arid lands. Springs of water burst out of the hillsides and cross the roads in rivulets. The road is through forests that a king might covet—oak and cedar and stately

fir. A valley where the cattle stand knee deep in grass has on one side a line of hills as desolate as Nevada; on the other side majestic slopes of pines.

Among the possible places for our purposes, a "bench" on Mr. Cook's ranch seemed especially attractive and caused me to consider most carefully the question whether it would be advisable to bring the equipment to Palomar. Your Committee has had in my reports descriptions of this place in some detail, and a balancing of the advantages and disadvantages of this site, the most promising in the San Diego section, as against Mount Wilson, the most promising in the Los Angeles region. This bench is situated at the southern edge of the broad, rolling, open space known as Dyche valley. Its contours adapt it admirably for all the requirements of an observatory site. Covered as it is with a good stand of maturing grain and fringed with magnificent oaks, its appearance is most inviting. Springs of water are found in nearby knolls above it, and a hundred feet or so below its edge an abundant stream breaks out that is said to run without change the year round. However, with the long California summer, no amount of irrigation could control the wide pasture lands and secure the changeless green surface of Mount Wilson. A few miles to the east, and less than 2,000 feet below, stretches Valle de San Jose, which heats like an oven and leads away toward the desert. The east winds in winter are said to be furious and not infrequent. The summer climate is almost uniformly pleasant, and the nights are always cool. The school session is a summer one, for snows in winter may block the roads and render them impassable for children. On the days that I was there the sea breeze was perceptible as early, certainly, as six in the morning, and it grew strong by noon. However, the bench I have described seemed especially sheltered by neighboring hills on the northwest and on the east, and its trees showed very little effect of wind. As soon as the sun was down, a decided chill was noticeable, and though the days had been warm, Mr. Cook proceeded, as a matter of course, to build a fire. The dews are heavy here, as at Cuyamaca. These facts were not favorable to one's expectation of the best seeing. The remarkable stillness, the steady temperature, and the evergreen covering of Mount Wilson could not be found on Palomar, though my judgment, from this cursory examination, would lead me to expect steadier atmospheric conditions here than at Cuyamaca.

There remains one other important factor against Palomar as a site for an observing station—its extreme isolation. Escondido is the nearest town of any pretensions, and that is 35 miles away. There is a road down the western side of the mountain, laid out to be a 10 per cent grade, but constructed steeper in places. A road is contemplated down the southeast end of the mountain, from Mendenhall valley into Valle de San Jose. This has been surveyed and, I was informed, the construction ordered. Whether railroads will soon come nearer the mountain's base than they are at present is entirely uncertain.

There are enormous disadvantages in the way of developing the country back of San Diego because of its remoteness, its surface configuration, and the insufficiency of water. It seems to me improbable that the power, the accessibility, the contact with civilization, and the resources of a city like Los Angeles, which Mount Wilson has at hand, will ever come within reach of Palomar. Moreover, its nearness to the desert and the absence of protecting mountains to shield it from the winds that play between the heated interior and the sea make it far from probable that we should have here the equable conditions required for the highest grade of astronomical work.

On returning to Cuyamaca I found that the equipment previously packed was already on the way to San Diego. On arriving there I received instructions to return to the Los Angeles region. The equipment was shipped at once to Pasadena and sent by pack train up the Sierra Madre trail to the summit of Wilson peak.

At the close of two weeks' tests at this station a week was spent at Flagstaff, Arizona, before returning north, where, by the courtesy of Mr. Percival Lowell, the records and instruments of the Lowell Observatory were freely put at my disposal to gain a knowledge of the conditions in that section.

The 9-inch telescope has been used at the following stations: Mount Hamilton, Echo mountain, and Inspiration point on Mount Lowe, Cuyamaca in San Diego county, and Mount Wilson.

The instrument was set up on Mount Hamilton as soon as completed for the purpose of testing the lens and mechanical parts of the mounting. Comparative observations were made with the 9-inch, 12-inch, and 36-inch telescopes. Some observations of the sun were made, but most of the tests consisted of observations of the stars, and particularly of close double stars.

It is well known that the maximum efficiency of a very large telescope can be secured only under excellent atmospheric conditions. It is also true that a large aperture is required for the most critical differentiation of the various qualities of seeing. It may very well happen that what may appear to be very good seeing with a 6-inch telescope will not prove so with an instrument of the largest dimensions. Moreover, different kinds of work vary in their requirements. For example, with a comparatively short photographic telescope no perceptible difference in the results will be found on nights of excellent definition and on those when the seeing is only fairly good. Again, for meridian-circle work the most essential condition is a steady image, and for difficult double-star work there is the additional requirement of fine definition.

Even though some classes of work may be carried on very successfully under circumstances which are not altogether favorable, it is nevertheless true that they would be more easy of accomplishment under excellent conditions; and it is also true that many important investigations require the highest obtainable efficiency of the most powerful telescopes for their successful prosecution. That site, therefore, which affords excellent seeing the most continuously, and the necessities, conveniences, and comforts of life the most abundantly, will be the best adapted for the needs of a great observatory.

For several years I have used the 36-inch and 12-inch telescopes of the Lick Observatory for double-star and other observational work, and on many occasions when working with one instrument I have gone to the other in order to make a comparison of the two. Whenever the seeing appears excellent with the large telescope it also appears excellent with the smaller one; but when it appears only fairly good with the 36-inch there is a tendency to rate it somewhat higher with the 12-inch. As soon as the tests began with the 9-inch it was further noted that there is a very perceptible difference between it and the 12-inch in the same direction. It was my especial object while the instrument was set up at Mount Hamilton to become acquainted with the characteristics of the image formed with the 9-inch under varying conditions, in order that I might properly interpret them and retain as far as possible 36-inch standards of excellence while making the tests in the various localities. To have proceeded in the opposite direction, by capping down the larger instruments to a 9-inch aperture would, in my opinion, have resulted in a lowering of the standard which it is

eminently desirable to maintain. It is manifestly impossible to give a numerical rating of the seeing which shall indicate its characteristics fully and without ambiguity. Nevertheless, it is convenient to use a number to express one's estimate of the effect of atmospheric conditions taken as a whole. The scale which I have used in this way embraces the numbers 1 to 5, inclusive.

The number 5 is used to denote seeing perfect in every way, a standard of excellence that seldom obtains; 4, excellent seeing with the excellent conditions lasting for considerable intervals; 3, good seeing, but with the images less sharply defined than indicated by 4, or having such conditions lasting for much shorter intervals; 2, poor seeing, images unsteady, large, or blurred, yet of such quality that good work of some kinds can be done, but not that requiring fine definition; 1, seeing so bad that good work with large instruments is out of the question.

At Lowe Observatory the 9-inch telescope was erected on April 23, between the observatory and the reservoir, and was dismounted on May 2. Observations of the sun were made during the forenoon, and tests of seeing at night—till midnight or later. On one forenoon only was the day seeing excellent. The first observations were made shortly after 6 a. m. At that time the sun's limb was sharply defined, and the granulations of the surface very clear and distinct. Several groups of sun spots were visible, and much detail could be clearly seen in both umbræ and penumbræ. At the time of the earliest observation the sun's image was remarkably free from passing heat waves, but this condition did not continue throughout the forenoon. The seeing gradually became worse, and by noon it was bad. It was the usual experience here to find the day seeing grow worse as the forenoon advanced, probably owing to radiation from the nearby and but scantily covered slopes of the mountain.

The night conditions at Lowe Observatory were on the average very much better than those prevailing during the day. Tests were made on all clear nights. Several were found to be excellent, others good, and only two bad, one of these being the night after leaving Inspiration point, when a violent storm was coming on.

In order to familiarize myself with 16-inch telescope conditions, I employed that instrument on parts of five nights in looking for new double stars. On each of these nights tests were made also with the 9-inch telescope. The following pairs, thought to be new, were found:

DM + 48°	1707	$8^h\ 49^m\ 16^s.3$	+ 48° 35′.9	0.9	10 and 10, Comp. of $0\Sigma 196$	
48	1716	8 53 16.6	+ 48 14 .5	0.3	8.5	8.8
50	1605	8 54 48.1	+ 50 28 .6	3.0	9.1.................	
51	1482	8 56 25.6	+ 51 12 .7	0.4	8.8	9.0
50	2174	15 19 25.3	+ 50 2 .2	3	8.2	12
50	2178	15 23 18.8	+ 50 49 .7	1	7.3	12
49	2408	15 31 40.2	+ 49 16 .9	0.4	8.2	8.6
51	2030	15 42 21.5	+ 51 14 .3	0.3	8.5	8.5
51	2077	16 15 57.9	+ 51 54 .7	3.0	8.9....................	
51	2105	16 24 22.1	+ 51 54 .1	3	7.3	12
51	2106	16 25 1.2	+ 51 45 .4	4	6.2	13
51	2130	16 40 10.9	+ 51 48 .7	¼	7.5	7.5

I have not secured measures of these pairs. The first on the list is of special interest on account of its being the companion of a well-known third-magnitude double star, viz., 9 τ Ursæ Majoris, or $0\Sigma 196$.

Alpine Tavern is situated among the trees near the head of a canyon. In its immediate vicinity there is no place that commands a clear horizon. A broad trail leads from it to Inspiration point, and narrow trails run up the mountain to the summit. No pack animals are kept at Alpine Tavern, and on this account it was not feasible to take the equipment to the summit of Mount Lowe. The only place within reach of the tavern that seemed suitable for the erection of the telescope was Inspiration point. The first observations were made here on May 7 and the last observations were made May 13, just before the telescope was dismounted.

At Inspiration point observations of the sun were made at intervals throughout the forenoon and the earlier portions of the afternoon, the first ones usually about 6.30 a. m.

It was found that the day seeing here is similar to that at the Lowe Observatory. Early in the morning the seeing was sometimes good, but it never had that excellent quality which was noted on one of the mornings at Echo mountain. During the forenoon the seeing always became worse, and in the afternoon it was never good. The mountain side to the east of this station was burned over only a few years ago, and it has not become covered with chaparral again. There is much surface exposed, and it is probable that the radiation from this is one of the causes of the seeing becoming worse as the day advanced.

Tests of the night seeing were also made, but they were not continued later than midnight. Some of the nights were excellent, others were fair, and none of them were very bad. It was possible

on three of the nights to take the car to Echo mountain and make additional tests with the 16-inch telescope.

The top of the Incline and the Lowe Observatory can be seen from Inspiration point. During my stay at the higher station I was constantly above the fogs which had hindered me so much at Echo mountain. They did not, during this period, rise to my elevation, 4,500 feet, but very often did cover Lowe Observatory all day long.

The telescope was erected at Cuyamaca on May 27, and taken down on the afternoon of June 5. Observations were made at intervals during the day, from shortly after six o'clock in the morning till the middle of the afternoon, and then again during the earlier portion of the nights. The results were not at all favorable. The seeing was nearly always poor, occasionally good, but never excellent.

The San Diego Flume Company has kept a record of the weather conditions at Cuyamaca dam almost continuously since 1888. I made some examination of this record, and from it derived considerable information respecting the weather conditions which prevail in this region.

The readings of the maximum and minimum thermometers show that the daily range of temperature is usually large, and that the weather sometimes becomes very cold in winter and very hot in summer. In winter the temperature occasionally falls several degrees below zero, and in summer it may rise above 100°. Thus, in June, 1899, there were five successive days with maximum temperatures of 104, 108, 110, 105, and 101. The minimum temperatures for the same days were 50, 46, 55, 55, and 54 degrees. The highest temperature noted in the record is 113 degrees.

The rainfall is heavier at Cuyamaca than at any other place in San Diego county. The heaviest snows also occur here upon the peaks, sometimes amounting to a depth of several feet. Even about the lake, in the little valley among the peaks, the snows are heavy. At one time last winter 30 inches lay upon the ground for six weeks, and ice formed on the lake to a depth of 8 inches. Here, as in other regions adjacent to the desert, water cannot always be counted upon from the melting of the snow. A warm dry wind may lick up the moisture and carry it away, leaving scarcely a trace.

The heaviest rains at Cuyamaca come in winter, but there are also local thunder showers in summer. During the last days of my stay in this region, I saw at a distance a number of these showers. It is said, however, that they are sometimes productive of very heavy rains. The prevalence of cloudiness in connection with these

showers and the disturbed atmospheric conditions indicated would without doubt prove an obstacle to the prosecution of solar work in this region.

The thermographic record obtained by the expedition at Cuyamaca extends over twelve days, from May 27 to June 8, inclusive. The temperature curve for this period is generally quite smooth and free from rapid alternations, but has, as a rule, a large daily range. This sometimes amounts to as much as 35°, passing in one instance from 46° at 6 a. m. to 81° at 4 p. m., and in another from 43° at 5 a. m. to 78° at 5 p. m. It was particularly noted that the only times of constant temperature were those when a strong sea breeze was blowing. Nevertheless, at such times, the seeing was invariably poor. The temperature curve at night was generally steep, often having a declining rate of from 3 to 4 degrees an hour. Only once was there a period of constant temperature at night. This lasted about four hours, with a variation of about 1°.

A thermograph was placed on North Cuyamaca peak on May 29, and allowed to remain there in a suitable shelter until June 8, thus giving a record for ten days. This record shows that more frequent small alternations of temperature were taking place on the summit than at the lower station, producing a less regular curve, especially during the day. The daily range in temperature on the summit was, however, much smaller. As a rule, it was less than 20°; the maximum was 29°. The night temperatures on the summit were quite satisfactory. On most of the nights there were intervals of several hours during which the temperature remained nearly constant or fell very slowly.

During the time I was at Cuyamaca the dews were heavy. This is a normal summer condition, due to the rapid fall of temperature at night.

The observations at Echo mountain and Inspiration point on Mount Lowe had shown that excellent night seeing prevails there. The day seeing there was also good at times, but it did not have that constant excellence which was deemed desirable.

Mount Wilson is only 3 miles east of Mount Lowe, and there was every reason to think that it has the same excellent night conditions that prevail on Mount Lowe. On the other hand, it appeared probable that it would have better day conditions, owing to the control of insolation and radiation by its dense covering of evergreen trees and chaparral. On going to Mount Wilson, therefore, I was instructed to devote the greater portion of the time to testing the see-

ing during the day. As will be seen from the details given below, the day conditions were found to be excellent; very much better than at any station tested in the south. Following is a record of tests made:

Thursday, June 18.—The erection of the telescope was completed this afternoon, but not in time to make any observations on the sun. Tests were made on the stars during the earlier portions of the night. The seeing was only fair, about 3.

Friday, June 19.—No tests were made during the forenoon. Tests made upon the sun during the afternoon showed the seeing to be good until 5.30 p. m., when observations were discontinued.

Saturday, June 20.—Tests were made on the sun at intervals from about 8.30 in the morning until nearly 6 in the afternoon. The seeing was fair in the morning, good in the early afternoon, and very good between 4 and 5 o'clock in the afternoon. Even at 6 p. m. it was nearly as good as the best day seeing we had at Cuyamaca. Very little wind.

Sunday, June 21.—Tests were made at frequent intervals throughout the day, beginning at 6.20 a. m. During the forenoon a pronounced sea breeze was blowing, the air had a chilly feeling, and the seeing was bad. In the afternoon the wind had decreased to a very light breeze, and the seeing was greatly improved. The sun spots have today been undergoing rapid changes. Some of these changes could be appreciated without difficulty or uncertainty after an interval of only a few minutes. From 3 to 4 o'clock the seeing was at its best. The granulations of the surface, the detail in the spots—both in umbræ and penumbræ—were then very clear and steady. The appearance at this time may best be described by saying that the effect as to clearness, sharpness, and steadiness was very like that of a print from a steel engraving.

At 4.30 the seeing was not so good as between 3 and 4 o'clock, but still the details in the umbræ and penumbræ of the spots, the filamentary character of the umbræ and the granulations of the sun's surface were very distinct, and the periods of steadiness lasted for considerable intervals. Between 5.30 and 5.45 the seeing was not so good as earlier in the afternoon. The image was somewhat less steady, and occasionally considerably blurred.

No observations were made at night. There was a light wind at Martin's camp during the evening, probably less than 10 miles an hour.

Monday, June 22.—The sun was examined from 6.30 to 7.15 a. m., and the seeing was found to be poor. A pretty strong west wind was blowing. During the forenoon the sky clouded and it remained cloudy until late in the afternoon. Unquestionably a storm condition prevailed during the day, and no doubt its effects lasted into the night.

From 8 to 10 p. m. the seeing was fair, about 3. The stellar images were small and distinct, but with considerable motion and more or less continual breaking of the diffraction pattern. A strong northeast wind, perhaps 20 miles an hour, was blowing during the night.

Tuesday, June 23.—From 3 a. m. to 4.15—*i. e.*, dawn—the stellar images were much better than in the earlier portion of the night. The central disc of the image was small, distinct, and sharply defined, and the diffraction pattern was much more satisfactory.

Very early in the morning the wind went down almost completely, and when the sun was first examined, from 7 to 7.20 a. m., the image was found to be excellent. The granulations of the surface and the structure of the spots were very clearly defined and almost wholly free from blurring.

By 10.30 a. m. the seeing had gone to pieces. While the image was occasionally clear and distinct, this was by no means the normal condition. On the contrary, it was during the greater part of the time more or less completely blurred.

Between 11 and 12.10 the seeing was considerably improved. There were intervals when the definition was good, but longer periods when it was poor, and occasionally the blurring was bad.

Between 2 and 3 o'clock the seeing was poor. It clouded up about the middle of the afternoon and remained cloudy until shortly after dark. It then became beautifully clear. During the night there was a strong wind from the northwest.

Wednesday, June 24.—Early in the morning, 6 to 7.30, the seeing was not very good, nor was it very bad. There was more or less constant haziness without pronounced blurring. Much detail could be seen, but not very satisfactorily. It did not have the steel plate effect of the finest definition. From 9.30 to 11.45 the seeing was poor. In the afternoon it was fair between 4 and 5 o'clock and somewhat worse during the next hour.

In the evening, from 8 to 10.30, the seeing was good—3 to 4. This was also the case during the latter part of the night—*i. e.*, from 3 to 4.15 a. m.

Thursday, June 25.—Between 6.30 and 7.15 a. m. the seeing was excellent in every way, the image very clearly defined and almost wholly free from blurring—steel plate effect.

Between 9.30 a. m. and 12 m. the image was less satisfactory than early in the morning. The limb of the sun was not so steady, and blurring was more frequent and more pronounced.

In the afternoon, between 4 and 5.45, the seeing was good, but by no means equal to that which we had early in the morning. Professors Campbell and Hale arrived at Wilson peak this afternoon. Professor Hale rated the afternoon seeing at 4.

Friday, June 26.—Early in the morning, from 6.15 to 7.15, the seeing was very good, but not equal to that of yesterday morning, nor to that of last Sunday afternoon. Between 9 and noon it was less satisfactory. It was then fair and was given a rating of 3.

During the middle portion of the day it was very warm. No observations were made in the afternoon until about 4 o'clock. The seeing was then excellent. Professor Hale rated it at 4. It continued good until nearly 6 o'clock.

In the early part of the night the seeing was good, but not excellent. The central discs of the star images were small and distinct, but the diffraction patterns were more or less in motion.

Saturday, June 27.—Early in the morning, between 6 and 7.15, the seeing was excellent. Later, during the afternoon, it was fair. In the latter part of the afternoon it was found to be excellent again.

Sunday, June 28.—Greater part of the day spent in exploring the mountain in company with Professors Hale and Campbell, and Messrs. Staats, Holmes, and Lukens, of Pasadena.

About 8 o'clock the seeing was found to be very good, and it was also good in the middle of the afternoon.

Monday, June 29.—Partially cloudy most of the day. No observations made during the forenoon. At 3.30 p. m. the seeing was not good.

Tuesday, June 30.—Seeing good in the morning and also in the afternoon. Most of the day spent in photographic work. Seeing good in the evening.

Wednesday, July 1.—No observations were made during the forenoon. The seeing was excellent between 4 and 5 o'clock in the afternoon. In the evening a wind arose and the seeing went to pieces.

Thursday, July 2.—At 9 a. m. the seeing was good. There was some blurring and a little disturbance at the sun's edge, but these

were not very pronounced. The seeing was rated at 3. Telescope was dismounted during the forenoon.

The summit of Mount Wilson consists of perhaps 200 acres of more or less rolling ground, intersected by ravines of moderate depth, sparsely covered with splendid pines, with open grassy spaces of considerable extent, affording ample and excellent sites for buildings, large or small, with sufficient room to meet the needs of a large community. The summit proper is situated near the southwest point of this area. Here, in the vicinity of the old building known as the Casino, is, in my opinion, the best site for observatory purposes.

To the southwest of the Casino the ground drops away rapidly and is covered as far as one can see down into the canyon with a dense growth of chaparral, giving an evergreen covering that effectively shades the ground at all times. The spurs to the southeast of the summit are also largely covered with a growth of chaparral and in places also with a larger growth of oak and pine. In this direction, however, the covering is not always so complete as toward the southwest. This is due to the more precipitous character of the mountain side in this direction, making it less easy for vegetation to gain and retain a foothold. Nevertheless the whole barren areas are of comparatively small extent, and this is especially so in the vicinity of the Casino. Moreover, the barren areas near the Casino are not generally solid rock, but slides of decomposed granite, and over these slides it would be possible with intelligent care to foster a growth of chaparral which would eventually more or less completely cover them.

In this connection it is well to hold in mind the abundant supply of water that is within reach at Strain's camp, only a third of a mile north of the Casino and less than 300 feet lower. Here are wells which at the present time are constantly overflowing. Even after a series of dry seasons it was found that the upper of these wells alone would by pumping yield a constant supply of not less than 25 gallons a minute. This would be sufficient for the scientific and domestic purposes of a large observatory, and also for irrigation purposes, should irrigation ever be found desirable. Considering their nearness to the summit and the difficulties that are ordinarily encountered in finding water near the tops of mountains in southern California, these wells must be regarded as little short of phenomenal. If Mount Wilson should be selected as the site for an observatory, it would be eminently desirable that the Observatory

should control at least one of these wells and its immediate watershed.

It is not known how difficult it would be to obtain a large supply of water at any other place near the summit of Mount Wilson. It is certain that wells or tunnels at many places would prove failures, as did the first one that was designed to furnish a supply of water for Martin's camp. Martin's camp is, in an air line, about half a mile south of the summit, and 700 or 800 feet lower. It is now supplied by a tunnel on the west side of the mountain at a distance of perhaps 500 feet in altitude below the summit. It is possible that the resources of this tunnel might be materially increased by extending it further into the mountain, or by running drifts parallel to the mountain side. This, however, is by no means certain. The result of further tunneling would depend almost entirely upon the character and arrangement of the strata encountered in making these extensions, and what these would be in a granite mountain densely covered with brush is not easily foretold.

As it stands today, the tunnel that supplies Martin's camp does not seem to afford a sufficient supply of water for a large community, such as might eventually grow up around an important observatory, and it certainly would not furnish the abundant supply that would enable water to be used extensively for irrigation, which would be necessary, for a time at least, if an attempt were made to clothe the barren slides with a covering of chaparral. The greater distance of the present tunnel below the summit as compared with the wells at Strain's camp is a forceful argument in favor of the latter.

An undulating ridge connects Mount Wilson with San Gabriel peak. Upon this ridge, at a distance of perhaps half a mile from the Casino, is a summit locally known as Alta, which rises about 60 feet higher than Mount Wilson itself. Alta is not of large dimensions. If an observatory were placed upon Mount Wilson it would be the most favorable site available for the storage of water. The pressure obtained from a reservoir placed here would not be very great, but would be enough to make it of extreme importance as a protection against fires and as a distributing center, etc.

While in southern California, from April 17 to July 19 (with the exception of the interval from July 8 to 16, when I was in Arizona), I watched the drift of ocean fog over the land in reference to its bearing upon the height at which an observatory would have to be placed in order to be above its prevailing higher levels.

In the Los Angeles region it soon became apparent that the height of the fog at this season varies greatly on different days. One morning it will be lying almost in contact with the ground and not advancing far from the sea. More frequently, however, the upper level has an elevation of from 3,000 to 4,000 feet, only occasionally exceeding the latter altitude.

At Echo mountain the upper surface of the fog was sometimes just below the Lowe Observatory. Whenever it was very near, the seeing seemed to be unfavorably affected. When it was a thousand feet or more below, no manifest influence was noted, either for good or bad.

The same characteristic was noted at Inspiration point. Here, also, when the fog rose in the canyons to an elevation nearly equal to that of my station, the seeing appeared to be unfavorably affected.

Mount Wilson is somewhat protected from the direct drift of the ocean fog by a spur of Mount Lowe, and during my stay at Mount Wilson the fog did not at any time approach very near the summit. At night currents of air were noticed descending the sides of the mountain, and no doubt there were ascending currents during the day. No ill effects from them were noticed.

In the San Diego region, near the sea, fog is, according to the United States Weather Bureau record, of daily occurrence during most of the year. In San Diego, in summer, it is generally cloudy at 5 a. m, and often so at 5 p. m. The middle portion of the day is usually clear, or partially so. The record indicates less fog in winter than in summer. The record does not contain any information respecting the altitude of the fogs. I was in San Diego county twenty-five days, from May 19 to June 13, at a season when fog is said to prevail to its greatest extent. On fifteen of these days my observations showed that the summit of San Miguel, whose height is 2,600 feet, was either below or in fog or cloud a portion or all of the day. On three days it was clear at the summit. On the remaining seven days I was not where the peak could be observed.

At Cuyamaca the ocean fog, blown in by strong westerly winds, reached my level on two occasions, and I several times saw it to the west, covering all the peaks in that direction lower than 3,500 feet.

The prevalence of high fogs in these regions makes it evident that an observatory should be placed at an elevation of more than 4,000 feet.

Many of the days and nights at Mount Wilson are nearly windless. Strong gales do sometimes blow ; some violent storms may be

expected every year, especially in winter ; but they are not numerous and are to be regarded as exceptional.

Altogether, I spent forty six days on these mountains. In that time there was one severe storm, when the wind was high—probably as much as 60 miles an hour. In addition to this, there were four or five other nights when the wind velocity rose to 25 or 30 miles an hour. Otherwise, the days and nights were very quiet. From all that I could learn, it appears that this record is not more favorable than the average.

Mount Wilson and Mount Lowe are on the southern edge of the San Gabriel mountains, and near the middle of the range from east to west. Here the range has its greatest breadth but not its greatest height. To the northwest of Mount Wilson there is a succession of ridges and peaks that rise to elevations of from 5,000 to 6,000 feet. But it is to the north, northeast, and east that the highest peaks of the range are situated. In these directions, beyond the San Gabriel river, numerous ridges may be seen, some more than 7,000 feet high, with peaks that tower above them, culminating in San Antonio (Old Baldy), 10,080 feet above the sea. These higher ridges lie directly between Mount Wilson and the Mohave desert. By reason of their situation and height they no doubt greatly protect Mount Wilson from winds that would otherwise blow across this region between the desert and the sea.

These higher peaks and ridges are from 10 to 25 miles away. Deep, broad canyons lie between, winding in and out among the mountains until they reach the valley on the south. The advantages of this arrangement from an astronomical standpoint will be readily understood. Streams of cold air settling down from these higher peaks cannot flood Mount Wilson. This source of bad seeing, therefore, appears to be wholly absent. San Gabriel peak is somewhat higher than Mount Wilson, but there is a deep col between them. Flooding with cold air appears to be a fruitful cause of bad seeing at some observatories. Thus Mr. A. E. Douglass speaks of the condition at Arequipa, Peru :

"The observatory is situated close to a river, down which on clear nights a swift stream of cold air descends. When this cold air reached the telescope, the seeing was immediately ruined. When this current was once established, no more good seeing could be expected for the remainder of the night." *

At Flagstaff also the temperature often falls suddenly in the latter

*American Met. Journal, vol. II, p. 395.

part of the night, and at such times the seeing is said to go to pieces.

The San Gabriel and San Bernardino mountains trend nearly east and west, and were it not for the depression between them at Cajon canyon, through which the Santa Fe Railroad passes, they would form a continuous range. The highest points in the San Bernardino mountains, viz., San Bernardino peak (10,630 feet) and San Gorgonio mountain (11,485 feet), are about 40 miles southeast of Cajon canyon. From these high peaks this range dips down toward Cajon canyon, which itself has an elevation of about 2,800 feet above the sea. The main portion of the Mohave desert lies north and northeast of the San Bernardino range. In the vicinity of Cajon pass it has an elevation of about 3,200 feet. Over the desert the air is subject to large and rapid variations of temperature; over the sea, only 70 miles away, it is not. The result of this is a movement of the air which follows the line of least resistance. This happens to be Cajon pass, noted in southern California as a very windy place. When the winds from the desert are strong they sweep violently through this pass southward toward the sea, in the direction of the Santa Ana mountains, and there the Santa Ana canyon affords a passage for them. It, too, is noted for its winds—so much so that in this section of the State any very strong wind is called a Santa Ana. An inspection of the map will show Mount Wilson far to one side of this usual track of strong winds. It is only when the pressure over the desert becomes so great that the volume of moving air cannot be passed through the dip in the range at Cajon canyon that the winds come across the mountains directly and sweep over Mount Wilson from the north.

Only a small portion of the Mohave desert lies directly north of the San Gabriel range. It sometimes happens that winds originating here sweep around the western end of the range through Soledad pass and thence southward through La Cañada. These winds strike Pasadena, Mount Lowe, and Mount Wilson from the northwest. The Southern Pacific Railway from Tehachapi to Los Angeles passes through Soledad pass.

The direction of the sea breeze through the San Gabriel valley is of course from west to east. When it is strong and meets a lighter current coming through Cajon canyon from the Mohave desert, it prevails and swings the latter around the San Bernardino mountains, and both then travel on through the San Gorgonio pass into the Colorado desert.

The sea breeze in the San Gabriel valley is usually strong and is felt almost every day during summer. But, quite unexpectedly, on Mount Lowe and Mount Wilson, overlooking this valley, it is always very light. On almost all of the days I spent on these mountains there were sea breezes, very gentle, fresh, but not cold. At such an elevation, with the ocean only a few miles away to the south, and nothing but low hills between, I should have expected much stronger winds from this quarter. The only suggestion I have to offer as a possible explanation is that the mountains to the west and northwest may be a sufficiently diverting barrier to give these winds a course that passes clear of Mount Wilson, and that the higher ridges to the northeast of Mount Wilson form a barrier to currents that otherwise would pass directly to the desert from the sea. It is 80 miles from Mount Wilson westward to the ocean. In this direction protection is afforded by the Verdugo and Santa Monica mountains, which, though lower than the San Gabriel range, are nevertheless sufficiently high to have considerable influence upon the winds. Moreover, it is probable that they help to divert west winds toward the south, causing them to pass to one side of Mount Wilson.

To the west-northwest of Mount Wilson there are many mountains, reaching all the way to Point Conception, 150 miles away; and to the northwest, further inland, are still more extensive ranges. It cannot be doubted that these ranges have an influence on the winds that come from their direction, tempering, deflecting, or even turning them completely aside. In any event, it is pretty certain that they would find an easier passage from the sea to the desert than by passing over the San Gabriel range.

In this connection we may consider the dust in the air over the Los Angeles plains, for this is very intimately connected with the winds that blow from the Mohave desert to the sea. When the desert is dry—as it is during much of the year, and particularly in summer—and strong winds blow over it, clouds of dust are raised high into the air. When this dust laden air passes through Cajon canyon and out to the sea, the air over the Los Angeles plains becomes charged with dust. In general, the dust filled region of the air has a clearly defined upper limit, here called the dust line, having an elevation of from 3,000 to 5,000 feet, sometimes falling lower, at others rising higher, according to the force and height of the winds. The air above the dust line is not entirely free from dust. Here, as in most other places, there is some dust even at very great elevations. Perhaps no place of moderate height in the temperate zones can be found that is wholly free from it.

During my stay in the San Gabriel mountains I watched the dust line closely. It often rose higher than I was accustomed to see it near the coast in central California. It became evident that an observatory at an elevation of less than 4,000 feet would often be below it, but that it does not usually rise much higher than this. Exceptions, however, do occur. At times it rises to 6,000 feet or more. These appear to correspond to the times when great volumes of air are moving from the desert to the sea, volumes so great that they cannot be passed through the dip in the range at Cajon canyon, and must perforce go over the mountains. This happened once during my stay at Mount Wilson. After some hot days in the valley a strong wind came over the mountain from the north, and the dust line, which before had been high, now rose above the peaks in the immediate vicinity. This lasted for a few days, and then the dust cleared from the valley, and the houses in it were again easily in view. Never a day passed while I was there that Grayback and San Gorgonio, the highest peaks in the San Bernardino range, were not visible. Again I could see without difficulty the lower mountains to the southeast—Palomar, the three Cuyamaca peaks, and their lower neighbors, some of them fully 120 miles away. Views such as this are not exceptional. There are some days when the dust rises high and thick, but according to the most reliable information, this does not happen many times in the year, and it certainly did not happen during my stay.

The summit of Mount Wilson is situated in the southwest quarter of section 29, T. 2 N., R. 11 W., San Bernardino base line and meridian. This section is at present owned by the Pasadena and Mount Wilson Toll Road Company, of which corporation Mr. Wm. R. Staats and Mr. J. H. Holmes, both of Pasadena, at present hold a majority of the stock. This company also owns the west half of the southeast quarter of section 30, and the west half of the northwest quarter of section 32, these being the sections which join section 29 on the west and south respectively. This company also owns the toll trail from the foot of Eaton canyon to the summit. All other lands in the immediate vicinity of the summit are owned and controlled by the United States Government, and form part of the San Gabriel Forest Reserve. Messrs. Staats and Holmes, Professors Campbell and Hale of your Committee, and Mr. T. P. Lukens visited Mount Wilson while I was there, and during their stay a number of questions which would arise in practical form if it were decided to place an observatory on Mount Wilson were dis-

cussed. From these discussions it was learned that the present owners would readily transfer a part of the mountain top, including the summit proper, for the use of an observatory; that they would assist in maintaining and fostering the growth of trees and other vegetation; that they would in every way safeguard the purity and permanency of the water supply; and, finally, that they would use their influence to make the summit more accessible by having an electric railway constructed to the mountain top.

The Pasadena trail, owned by the Toll Company, extends from the summit of Mount Wilson to the foot of Eaton canyon, a distance of 9 miles. It is constructed on a grade which is said to be 10 per cent. in most places, but nowhere exceeding this. The trail was located with the idea that it should eventually be converted into a wagon road. This could be done without much difficulty in most places. Portions of it, however, would require rock excavations and the construction of retaining walls, and some bridges might be necessary to avoid some of the present abrupt turns.

The fact that all the land excepting the portions enumerated above is held and controlled by the United States as part of a Forest Reserve is a very important consideration. It insures a constant patrol of the adjacent regions by the forest rangers, a safeguarding against forest fires, which are at times very destructive in semi-arid regions; a regeneration of the forest trees; a saving of the deep accumulation of humus, and thereby a conservation of the water—all matters of vital importance to the well being of an observatory whose work is to be largely solar research.

The San Bernardino mountains have been mentioned as lying to the east of the San Gabriel range, and as culminating in the San Bernardino and San Gorgonio peaks, about 40 miles southeast of Cajon canyon, which separates the two ranges. I did not visit these mountains, but from the time of my arrival in southern California until I came away I made many inquiries and heard much concerning them, particularly from those who have lived in them and made a study of their characteristics. From all the information that I could gather from the most reliable sources it did not seem probable that I should find in these mountains any location which would have conditions favorable to a high grade of astronomical work—conditions comparable to those that exist in the San Gabriel mountains. In reference to these matters I have particularly consulted with Mr. Lukens. In his opinion there is only one region in the San Bernardino mountains that would be at all worthy of consideration, and that would

not have advantages at all comparable with those of Mount Wilson. This place is Rogers cliff, on the Arrowhead grade, near Squirrel Inn Resort. It is said to be a delightful vicinity and one in which lumbering operations have been conducted intelligently. But the land is held in private ownership, and there is no assurance of the timber being renewed, as on the Government reserves. Sheep, which have devastated large areas in other sections, have been kept out of this one. The elevation is about 5,500 feet.

A large portion of the Mohave desert is immediately north of the San Bernardino mountains, and the Conchilla and Colorado deserts lie to the southeast. In the midst of the range, at an elevation of from 5,000 to 7,000 feet, there are valleys of considerable extent, the most noted of which are Bear valley and Little Bear valley. The former lies directly north of the highest peaks of the range and about 10 miles from their summits. The ridges to the north generally rise not more than a few hundred feet above the floor of the valley before they begin their rapid descent into the desert, which here reaches an elevation of about 4,000 feet. In winter snow often covers the ground to a depth of several feet, but when it melts a desert wind may carry away the moisture, leaving very little water. The summer days are warm, or even hot, and the nights are always cold. Even on summer nights it often freezes.

Little Bear valley lies farther west and has an elevation of about 5,500 feet. It has no considerable ridges between it and the Mohave desert, which lies 2,000 feet below it and only a few miles away. Mr. Lukens states that he camped for two weeks in this valley in June, 1899. At that time the temperature rose into the eighties during the day and fell below freezing every night. From temperature considerations alone, it is evident that these valleys would not be desirable locations for an observatory.

The San Bernardino Forest Reserve covers the San Bernardino mountain region, but the Government does not now own or control the best portions of the forested areas. Before the region was set apart as a reserve, private and corporate owners had secured control of practically all the valuable timber in accessible locations. Lumbering operations have been extensive. Logs are sawed at the mills and the lumber is hauled down to the plains. Roads have been necessary for these operations. The steep slopes of the mountains from the plains on the south to the crest of the ridge where the timber and mills are situated made it very expensive to secure easy grades for the various roads that connect the two sections. These are all

toll roads, owned, at least in part, by the same corporations that control the bulk of the valuable timber of the reserve. By making the rates of toll on timber over these roads practically prohibitory, these corporations have made it impossible for any one to conduct lumbering operations except under their auspices or by their permission. In this connection the following statement, taken from a report by Mr. John B. Leiberg* on the San Bernardino Forest Reserve, is of interest:

"There is, however, a point to which I desire to call attention. This relates to the means of communication with the forest areas from the plains. On the Mohave side of the uplift a number of roads and trails lead into the reserve. These roads are free and communication over them with the interior of the reserve is easy. On the western slope the only wagon roads leading into the forested tracts are toll roads owned by private corporations. The section of road from Highland Mill to Bear valley, along the crest of the ridge, is likewise a toll road. These corporations charge persons traveling on Government service toll, or not, according to their pleasure. The Arrowhead Company customarily refunds the toll or gives a pass upon proper application; but not so with the Highland Company, controlling the City Creek Road. Considering the fact that the different corporations own the bulk of the commercially valuable timber which grows in the reserve, and that the Government, in patrolling the reserve in general, protects this timber as well, it would be but a matter of equity that officers of the Government should at all times be entitled to the free passage of these roads."

As a result of the lumbering operations, the roads that lead into these forest regions are much traveled, and in summer they become very dusty. Some of them have much clayey road material, which grinds into very fine powdery dust. Parts of the City Creek trail are of this character. It has been said that "in all the State of California there is not another road so dusty."

I am not in possession of much information concerning the winds in the San Bernardino mountains. It is always stated, however, by those whom I have heard express any opinion in the matter, that winds prevail there to a greater extent than in the San Gabriel range. From what has already been said of the winds in the vicinity of the Cajon pass, and from the proximity to the Mohave desert, we should reasonably expect more wind, at least in the western and more exposed parts of the San Bernardino range, and, along with it, much desert dust high in air.

*U. S. Geol. Survey, Twentieth Ann. Rept. 1898–99, part V, Forest Reserves, p. 454.

To sum up: In the San Bernardino mountains there would in all probability be more wind and more dust, and certainly much greater daily range of temperature and less contact with civilization, than in the San Gabriel mountains. On the other hand, they are much less steep, and already have graded wagon roads to the crests of their ridges.

San Jacinto peak rises 10,805 feet above the sea. On its eastern side it is said to be the highest vertical wall in the United States. From the summit one looks over the San Bernardino plains to the west and over the Colorado desert to the east.

Strawberry valley lies southwest of the peak, half-way down, 5 miles away in a straight line, but 12 miles by the trails that one must take. It is well covered with pines and oaks, and is in the midst of many thousand acres of similar timber. It has numerous springs of pure water and several streams that never fail. The valley is wide and open, and is said to have numerous small elevations within its limits which rise some hundreds of feet above its floor. Tahquitz valley (7,500 feet), Lily rock (7,973 feet), and Tahquitz peak (8,826 feet) overlook Strawberry valley from the east. From what we know of mountain temperatures, we should expect these higher elevations to cause large daily variations of temperature in the valley below them. In themselves these higher levels are too elevated and too inaccessible to be desirable sites for a permanent astronomical station.

Many earthquakes are felt in the San Jacinto mountains. Tahquitz peak is especially noted for them. This circumstance alone counts heavily against the region for astronomical purposes.

I did not make a personal inspection of the mountains in the vicinity of Santa Barbara. They were not considered when the work in southern California was outlined. Later the question arose whether a really fine site for an observatory could be found there, and I then made inquiries concerning them. I have particularly consulted Mr. Lukens, and the salient facts respecting these mountains are given in the following paragraphs:

The Santa Ynez mountain is a ridge several miles in length, about 5,000 feet in altitude, lying parallel to the coast and rising abruptly back of the city of Santa Barbara. Its summit is accessible by a trail, and its air line distance from the sea is only 5 miles. Water is not found anywhere near the summit. There is but little timber on the mountain, and only a fair covering of chaparral—not nearly

so much as there is on Mount Lowe. For many years this has been an extensive sheep range.

The Santa Ynez river lies back of Santa Ynez mountain and is about 2,000 feet below the summit. It is the nearest source of water supply. It is said that the water supply of the city of Santa Barbara will eventually be taken from this river, as soon as the tunnel through the mountain is completed.

Pine mountain is farther inland. It rises to an elevation of about 6,000 feet, and is well covered with timber. The desert lies immediately north of it, and is only about 2,500 feet below the summit. Here there would be no protection from the desert dust and wind. Moreover, it is very inaccessible, as is the case with most of the mountains inland from Santa Barbara. At present there are no roads and no trails over which instruments could be taken.

The mountains north of Santa Barbara are more promising than those to the southeast, and yet they do not have conditions as favorable as one would desire. It is pretty nearly an arid country about Santa Barbara. Toward the southeast it is even more barren than in the immediate vicinity of the city. This is a conglomerate region, in which water is exceedingly scarce.

In the beginning it was not intended that an examination of any portion of Arizona should be made. On his return to Chicago from Pasadena, Professor Hale was much impressed by the transparency of the air over the desert. This led him to urge a brief inspection of northern Arizona. For this inspection I made a trip to Flagstaff and remained a week at the Lowell Observatory—from the evening of July 7 to the morning of July 15.

In response to an inquiry by Professor Campbell, Director Lowell very kindly placed the 24-inch telescope at my disposal, and besides did everything possible to facilitate the work.

In the late afternoon and early evening, Mr. Lowell was using the 24-inch telescope on Venus and Mars. Mr. Slipher was away and the spectroscopic work was discontinued during his absence. This circumstance gave me a large measure of time with the telescope without interrupting the regular routine of the observatory.

I was at Flagstaff at an unfavorable season. For a month or two in the summer there are almost daily thunder showers in this elevated region. Some of these are heavy storms, as was that on the day I left; but more often they are quite local and of short duration. During the week I was there clouds usually formed in the forenoon, sometimes early in the morning, and apparently first in

the vicinity of the higher mountains, such as the San Francisco peaks, etc. In general the clouds covered only a part of the sky, and their drift was slow. Observations were frequently interrupted by them. During the middle of the day the cloudiness increased, and thunder storms followed. Late in the afternoon it would clear and the nights were generally cloudless.

As a result of these unsettled conditions it was not expected that the seeing would be at its best, and during my stay Mr. Lowell did not regard it so. To me it appeared only fair. At no time did I rate the seeing above 3 on my scale. However, a comparison of his standards of excellence and mine was interesting and useful.

I took with me to Flagstaff the helioscope loaned to the expedition by the Yerkes Observatory. Mr. Lowell very kindly had it fitted to the 24-inch telescope, thereby making direct observations of the sun possible. A number of very large sun spots were visible, and much detail in them could be seen at times, but the moments of steady images were of short duration. For the most part heat waves followed each other rapidly, causing much movement of the image and some blurring.

When observations of the sun were made with the large telescope the aperture was reduced by diaphragms placed in front of the objective to 9, 12, or 18 inches.

In addition to the direct observations, the sun's image was occasionally projected upon a sheet of paper, usually to a scale of about 3 feet to the sun's diameter. Most of the observations of the sun were made during the forenoon. The earliest ones were made about 7 a. m., and they were usually continued at intervals until about 1 p. m. On one of the days the sun was also observed about 5 p. m.

Observations of stars were made at night, after Mr. Lowell had completed his work on Mars, usually until about midnight. On the whole, the conditions during this portion of the night were much better than those during the day. Double stars having equal components and distances greater than 0.″25 were easily separated on most of the nights, and the measurement of much closer pairs would not have been difficult. Nevertheless, the seeing was not at any time of the highest quality. The diffraction pattern of a star's image was always more or less broken and in motion.

During my stay at the Lowell Observatory I detected two double stars, which appear to be new, and verified the duplicity of a third which I had suspected with the 16-inch telescope of the Lowe Observatory. These pairs are as follows:

D. M. + 51°	1808	$12^h\ 59^m\ 08^s.3$	+ 51° 46′.8	Close pair, companion of ε1718.
+ 48	2108	13 13 28.8	+ 48 32.6	8.5 and 11.0, Est. 1″.
+ 48	2243	14 44 15.6	+ 48 50.9	A close pair.

While we were at the Lowell Observatory the wind during the day was usually light and always from the southwest. Late in the afternoon it would die down and remain quiet for some hours. This is said to be the normal summer condition. Mr. Lowell states that at Flagstaff the best seeing of the day is usually at this turn of the tide—in the afternoon when the wind is dying down and at the corresponding period in the morning before it rises. This accords with the experience of Mr. A. E. Douglass, who has very kindly placed at my disposal the following statement concerning the seeing at Flagstaff from his forthcoming article on the "Selection of observatory sites":

"The seeing at Flagstaff, Arizona, is dependent on the general and local topographic features and the prevailing winds. The town is situated on the southwest margin of the great Rocky Mountain plateau. On account of the great size, elevation, and barrenness of that plateau, insolation and radiation are stronger than on the other desert regions touching it on the southwest. There is, therefore, an indraft toward this plateau from the southwest in the daytime, stronger in the afternoon owing to the insolation, and an overflow from the desert at night as the cold air settles down upon it, owing to radiation, reaching a maximum in the late night. The normal winds at Flagstaff, therefore, are southwest in the day and northeast at night. In summer, when the days are longer, the day condition prevails, and the prevailing clear-weather wind is southwest (changed to southeast when bringing the summer rains); in winter, with its length of night, the night condition dominates, and the prevailing clear-weather wind is northeast (changed to southwest in a storm area). It is the sunset and sunrise lulls between these day and night winds that give us the periods of best seeing.

"This explanation of the good seeing at sunset and sunrise has been verified in many ways. During the observations of Eros at the Lowell Observatory, when it was necessary to observe at least twice during the night, once early and once late, the late seeing on clear nights was always worse than the early, and it was usually accompanied by a current of air from the northeast. This is the overflow from the desert, as already explained. This overflow, coming on rather suddenly late in the night, is also the explanation of the sudden rise in temperature in the valley of Flagstaff in the summer, at three or four o'clock in the morning, frequently found on the thermograph records. Previous to this hour the quiet air in the valley of Flagstaff has been growing steadily colder; but when the overflow occurs in the form of a fresh northeast wind it

carries the settled cold air out of the valley and raises the temperature.

"The southwest margin of this desert plateau is a ridge called in part the Mogollon mesa. It follows the line of a great fault through Arizona from northwest to southeast. This ridge acts like a dam to the rising warm air by day and the down flowing cold air at night. Flagstaff is at one of the low points on this dam, and therefore gets a particularly pronounced effect from this interchange of air between the high and low desert regions. Its winds are probably stronger than those anywhere else along the ridge and they begin earlier and last later.

"Now as the cause of unsteady seeing is the multitude of irregularities of density in the air, produced by contact with heated or chilled surfaces, and as, also, these irregularities are brought over the telescope by aid of the wind, we may generalize and say that the badness of seeing increases with wind and the opportunity that wind has of contact with heated or chilled surfaces. We have seen that Flagstaff is particularly subject to strong winds as compared with other parts of Arizona; we have now to study the opportunity that wind has of contact with heated or chilled surfaces. It is perfectly evident that hills and mountains, reaching up into the great mass of moving air, present the best opportunity for such contact. On examination we find that the entire horizon of Flagstaff from north-northwest to east-northeast is occupied by a mountain range, rising from 2,000 to more than 5,000 feet above the town, and distant from it between 4 and 12 miles. The winds that pass these peaks are not only diverted in a vertical direction, which causes change in their density, but also, by contact with their warmed or chilled surfaces, suffer small irregularities in density throughout their mass. Hence, whenever the wind blows from a northerly direction the seeing becomes poor in proportion to the velocity of the wind and its directness of passage over the mountains. Therefore in winter, when the northeast wind predominates, the seeing is generally poor.

"This explanation of the bad seeing in winter has been frequently verified by testing on the same night the atmosphere at Flagstaff and at points as little as 12 miles east, out of the lee of the mountains. Even when the seeing at Flagstaff was frightfully bad, Eros showing a diameter of 13 seconds, the seeing away from the mountains was perfectly fine and steady.

"Thus the times of best seeing at Flagstaff are at sunset and sunrise in the non-winter months, and the poor seeing at other times results from the topographic features and the prevailing winds.

"It should be remembered that this criticism of Flagstaff seeing is derived from a minute comparison of it with other parts of the southwest arid regions. Compared with eastern localities I regard its qualities as very superior."

I took with me to Flagstaff one of the Draper thermographs belonging to the equipment of this expedition, and during the week

I exposed it in the thermograph shelter of the Lowell Observatory. The range of temperature shown by the record was 29°, from 59° to 88°. The greatest variation in any one day was 24°.

The record during the day was generally full of small variations, in addition to the larger changes. This was probably due to the passing of more or less extended clouds, giving intervals of sunshine and of shade, but no direct test of this hypothesis was made.

The temperature at night was more uniform than by day, but the night curve did not always have the smoothness that one likes to see. However, on some of the nights there would be an interval of five or six hours during which the change of temperature would amount to only one or two degrees.

Mr. Lowell very kindly placed at my disposal the thermographic record of the observatory. The time available did not permit a detailed study of the entire record, but I examined with some care the sheets for the year 1902.

Some of the general characteristics of the record were at once evident. The fluctuations during the daytime were often large, and superposed upon these were frequent minor variations. In passing from day to night temperatures there was, as is always the case, a pretty rapid change during the early hours of the night. This period usually lasted about three hours—to take the year through, from about 6 p. m. to 9 p. m., but earlier in winter and later in summer. This, however, was not the rule. After the early evening hours the temperature gradient would generally become less steep, or even nearly horizontal, save for minor fluctuations.

A feature worthy of special notice was prominent on many of the sheets. Often in the latter part of the night, generally after 3 a. m. and often as late as 6 a. m., the temperature would suddenly fall from 5° to 15°, and then after a short time rise again, partially regaining its former height. As an example, on the sheet I obtained there is shown on the morning of July 14 a sudden drop of about 7° shortly after 3 o'clock a. m., and half an hour later a rise, nearly as sudden, of about 4°. I have been told that these sudden changes of temperature are here associated invariably with very bad seeing, which is entirely in accord with what might be expected under such circumstances.

Respectfully submitted.

W. J. HUSSEY.

MOUNT HAMILTON, CAL.,
August, 1903.

Appendix B to Report of Committee on Observatories

LETTERS RELATING TO SOUTHERN AND SOLAR OBSERVATORIES.

CONTENTS.

I. CORRESPONDENCE RELATING TO PROPOSED SOUTHERN OBSERVATORY.

Extracts from Confidential Statement.

The following extracts from a confidential statement, prepared in June, 1903, are prefixed to the letters relative to the proposed Southern Observatory, in order to render more intelligible the comments upon the statement contained in the letters:

"The Advisory Committee on Astronomy for last year of the Carnegie Institution (Messrs. Pickering, Langley, Newcomb, Hale, and Boss) recommended the establishment of two observatories. One of these was proposed for the southern hemisphere, to assist in reducing the disparity which now exists relative to the observation of astronomical objects in the two hemispheres. The other is a proposed solar or astrophysical observatory, to be established in the best practicable atmospheric conditions and with a powerful equipment.

"The Institution is not in any way committed to either of these enterprises at present. * * * Accordingly a special commission of three astronomers (Messrs. Campbell, Hale, and Boss) was appointed to investigate and report more fully in regard to these recommendations. * * *

"Among the special observations that are regarded as important are the following:

(1) "A fundamental determination of star positions, with extension of these observations by secondary methods to include every star brighter than the seventh magnitude between —20° and the south pole. * * *

(2) "Observations for stellar parallax have been secured at the Cape Observatory. Against this one observatory we find several observatories in the northern hemisphere actively engaged in deter-

minations of stellar parallax. * * * Therefore, determinations of these has been considered an important feature for the work of the proposed observatory.

(3) * * * "It is considered highly desirable that another very large reflector should be provided for this use [determination of radial motion] in the southern hemisphere. Were such an instrument provided it could also be employed in other spectroscopic researches requiring powerful optical means and also in the photography of nebulæ. * * *

(4) "The extension from —32° to the south pole of zone observations upon the general plan adopted by the *Astronomische Gesellschaft*. The Cordoba Observatory has secured the necessary observations for the zone —22° to —32°. Only one fourth of the sky remains to be covered. * * *

(5) "It is thought desirable that extensive observations for variations of latitude should be made in the southern hemisphere somewhat upon the plan now adopted for the northern hemisphere in the international service. * * *

(6) "During recent years, and especially at the Lick Observatory, very thorough surveys of the stars in the northern sky have been carried out for discovery and measurement of close double stars. It is considered desirable that similar investigations should be carried out with at least one large telescope for that part of the southern sky which is beyond the reach of northern observatories. * * *

(7) "Eventually certain astrophysical researches of precision, requiring great optical power, will be required in the southern hemisphere to complete the evidence relative to certain classes of objects which have been, or are about to be, investigated in the northern hemisphere. * * *

"In view of these considerations, it would be of great service if the consensus of experienced astronomical opinion could be obtained in regard to the relative urgency of observations which ought to be undertaken now in the southern hemisphere and which are inadequately provided for at existing institutions there, whether such observations have been alluded to in this statement or not. * * *

"It would also be useful to learn whether any similar plans are known to be under consideration by others; or whether there is any prospect of such. * * *

"Letters have been addressed to astrophysicists in relation to the proposed astrophysical observatory for which a site in southern California is contemplated. The object of this observatory would be to investigate the physics of the sun, and, especially, the amount and variation of solar radiation, with apparatus more powerful than, and in conditions of atmosphere superior to, any which have been hitherto available for the purpose. Your suggestions and advice upon this head would also be very acceptable." * * *

Letters from Correspondents.

[*From Director H. H. Turner, of the University Observatory, Oxford, President of the Royal Astronomical Society.*]

Oxford, *July 17, 1903.*

The Statement deals chiefly with the project for a Southern Observatory, and this reply will be confined to that project. Before considering the points of detail raised in the statement I should like to express an emphatic opinion that there is now, as there has always been hitherto, a great need for assistance of astronomical observation in the southern hemisphere. Various spasmodic attempts have been made in the past to bring our knowledge of the southern hemisphere into better relation with that of the northern. Some of these have been successful, others have failed: and it will help us in considering the matter if we glance at a few instances of both kinds. As unsuccessful attempts I will quote—

1. The project initiated by a Committee of the Royal Society half a century ago (see Mon. Not., R. A. S., xiv, 129) for a large reflecting telescope in the southern hemisphere. The Melbourne telescope was provided, but very little has been done with it.

2. The project for a Southern Survey (see Mon. Not., xxiii, p. 147; xxv, p. 118; xxvii, p. 132; xxix, p. 161) which seemed to be making some progress, but eventually died out.

3. The southern share of the Astrographic Chart, which is lamentably behindhand at present; three of the southern observatories which originally undertook a share never made a start, and two of those which replaced them are in great need of assistance. At others the work is going on very slowly, except perhaps at the Cape Observatory.

For comparison with these we may cite various successful enterprises, and it will be seen that they are either of a purely expeditionary character or that at least the workers remained in touch with the northern hemisphere.

4. The expeditions of Halley and Sir John Herschel to the southern hemisphere, which successfully advanced our knowledge of that hemisphere at the epochs of the expeditions.

5. The expedition, in recent times, of Mr. McClean, who extended his spectroscopic survey of the bright stars to the whole sky and incidentally discovered oxygen in β Crucis.

6. The semi-permanent expedition from Harvard to Arequipa, the material collected at Arequipa being returned to Harvard for examination and discussion. Among other discoveries, this has given us several "new stars."

7. The work done by the late E. J. Stone in forming the Cape Catalogue, 1880, which was largely expeditionary in character. Mr. Stone remained at the Cape only 9 years, and went out definitely to do this single piece of work.

8. To some extent the same may be said of the work done by Dr. B. A. Gould at Cordoba.

9. In considering the eminently successful work done by Sir David Gill at the Cape Observatory, it may be remarked that he has paid frequent visits to Europe and kept himself in close touch with the northern hemisphere throughout. In the case of one large piece of work, the Cape Photographic Durchmusterung, the measurement of the plates was conducted by Professor Kapteyn in Europe.

No conscious selection of the above nine instances has been made, though it is quite possible that I have been unconsciously influenced by the opinion already indicated: that much, if not all, of the successful work done in the southern hemisphere has been accomplished either by observers who have made a purely temporary expedition from the northern hemisphere, or by men who have kept closely in touch with the North, or quite recently by the examination, in Europe or America, of material collected in the southern hemisphere.

And if this is true, it is, after all, only natural. The permanent observatories in the South have to contend with the gravest disadvantages. The governments at their backs are comparatively poor; there are heavy claims upon them for other work, such as meteorology (as at Sydney), or telegraphy (as at Adelaide), and they are far removed from the advantages of the older civilizations in respect of intercourse with men of science.

The lessons inculcated with regard to future projects are tolerably obvious, and I cordially welcome the general proposal to establish a Southern Observatory of the *expeditionary kind* indicated in the statement. I proceed to consider the points of detail dealt with in the statement.

The Observations Considered Important.

1.* *Urgently Needed.*—Personally I should like to see the photographic method used. I have elsewhere indicated (Mon. Not.

* The figures refer to the numbered paragraphs in the Confidential Statement, pp. 106-7.

R. A. S. LVII, p. 349) the lines on which I feel sure a simple and efficient photographic transit circle could be constructed. The heavy work of the Astrographic Chart has compelled me to defer any trial of this method, but having nearly completed our measures we are now on the point of trying it, and possibly information as to its success may be forthcoming at an early date. The "magnitude equation" is so troublesome for eye observations that I feel sure we ought not to delay the introduction of photography into meridian work any longer, and I feel confident of the success of the method suggested. No new departure is contemplated beyond mounting a mirror on pivots instead of providing a telescope, and this should offer no difficulty—rather the contrary. But I am well aware that no instrument is complete so long as it is only on paper.

2, 3, 4, 5, and 7. I thoroughly agree with what is said and have no special remarks to make.

6. *Double Stars.*—The work of Mr. R. T. A. Innes at the Cape Observatory should be remembered; but Mr. Innes has recently been appointed director of a new observatory where this work may not be possible, since the first call on his energies will be from the *meteorological side*. Compare what has already been said about the non-astronomical calls on the attention of directors of southern observatories.

Other Observations not Mentioned in the "Statement."

8. *Planetary observations of position* from the southern hemisphere are in some ways as desirable as stellar. Sir David Gill has already started observations which will supply several of these wants; but I should like to draw attention to the fact that a series of observations of the moon which would give good places *throughout the lunation* during at least one year would certainly improve our knowledge of the parallactic inequality; and for completeness we should have series obtained at both northern and southern stations, unless a series could be obtained from near the equator, as Madras or Kodaikanal. The chief requisite is a *fine climate*, such that the moon could be caught very near new moon and pretty continuously in at least one or two months.

There are various methods which might be used; to give one example, photographs might be taken with a telescope clamped for the night (*a*) of the moon and (*b*) of stars preceding or following by a known interval. These photographs could be measured and discussed elsewhere if necessary.

9. At some time a number of longitudes of stations in the southern hemisphere must be determined with modern precision.

While not immediately pressing, this work might become pressing at any moment; and it could scarcely fail to be of ultimate service if a new station were fixed relatively to (say) the Cape Observatory with accuracy.

Similar Plans under Consideration.

I know of no similar plans. On the other hand I have had special reasons for fearing that the total available energy of southern observatories will for the next twenty years be almost completely absorbed by the work of the Astrographic Chart. On this point I have already addressed a letter to the Chairman of the Committee, and perhaps I need not here repeat what I said.

Location.

I have made inquiries about

(*a*) *South America*, which would in many ways be suitable, as differing in longitude from existing observatories. For stations of considerable southern latitude the replies have not been favorable.

(*b*) *South Africa.*—I hear very good accounts from Sir David Gill and Mr. Innes of the site selected by the latter for the new observatory of the Transvaal.

(*c*) *Australia.*—Mr. Russell gives good accounts of sites near Sydney. Recently I have heard sites near Hobart Town, in Tasmania, much commended. The climate is said to be beautiful and land so cheap that a large tract could be secured.

General Plan of the Work Temporary.

I have already commented upon this point at the beginning of this letter. To keep in view the "accomplishment of a few specific works, the conclusion of which could be foreseen within a limited term of years," seems to me to be exactly the way to set about the advancement of the astronomy of the southern hemisphere, if we may trust previous experience; and it seems to me further quite probable that there are several men, perhaps many men, of proved capacity and experience, who would find an expedition to the southern hemisphere desirable and delightful if it could be undertaken without financial loss and for the completion of a piece of work already carried out in the northern hemisphere. I understand that this kind

of expedition is in the minds of the Committee, and it seems to me to be most attractive, and to promise an economical solution of the problem of dealing adequately with the southern hemisphere on lines suggested by a study of past history.

[*From Director M. Loewy, of the Paris Observatory, President of the Congress for the Carte du Ciel.*]

[Translation.]

PARIS, *July 20, 1903.*

The establishment of two observatories in the southern hemisphere would be, without any doubt, of high utility for astronomy.

The program prepared by the Committee in regard to the work to be accomplished there, seems to me, in its broad lines, most judicious. You have expressed in your communication two ideas which are in perfect harmony with my own personal view: First, that large scientific enterprises often stand in intimate relation with each other, which brings it to pass that the success of the one instigates progress in the others; Second, that it is desirable to have at our disposal a large number of carefully determined proper motions in order to undertake with profit some of the noblest problems which today are occupying the minds of the most eminent astronomers. It is only in this way that one can acquire more precise conceptions of the nature of the trajectory of our solar system and of the general structure of the universe. I take this point of view in communicating to you the reflections suggested by a perusal of the program of the committee.

First of all, I should like to suggest a slight modification of your plan of work concerning the execution of the meridian observations between $-32°$ and $-90°$ of declination, in order to give to this research a more general utility and to render it valuable in the construction of the Astrographic Chart. In order that you may better appreciate the drift of the remarks which I submit to you, allow me to present some explanations concerning the details connected with this work. The recent researches which I have published on the subject of the precision with which one can take the astronomical coordinates of the stars from a photograph show that by exercising the necessary precaution one can attain such precision that the total probable error would reach at the maximum $\pm 0.''2$ even for the faintest images. This determination is not in the given instance an *optimistic* estimate; it represents the reality. It is well to add that

a *great part* of the probable error above indicated springs from the inexactitude with which the positions of the comparison stars, chosen for the calculation of the constants of the plates, are affected.

From this one may conclude that if the photographic observations should be repeated twenty years from now under the same conditions of precision we should be able to determine with accuracy the proper motions of about 100,000 stars, as I have indicated on a former occasion.

Besides, in that which concerns the construction of the Astrographic Chart, which includes the exact positions of more than 3,000,000 stars, twelve observatories out of the eighteen in this line of work have not only completed their observations, but have also concluded the task of measuring the rectilinear coordinates that correspond to them.

One of the essential requirements under which the degree of approximation mentioned above could be equaled, and even surpassed, lies in the determination of the plate-constants with all possible accuracy. In investigating this question, the majority of the observatories associated in this work have decided that, if the positions of the comparison stars should be taken from the catalogue of the Astronomische Gesellschaft, their accuracy would be insufficient and would not correspond with that which obtains in the measurement of the rectilinear coordinates. It is for this reason that out of the twelve observatories above mentioned seven proceeded to the direct determination of their comparison stars with the help of precise meridian observations.

It seems to me, then, that the general catalogue which you are proposing to observe between $-32°$ and $-90°$ would have a much higher value if, aside from all other applications, it could supply the comparison stars necessary for the reduction of the plates of the Astrographic Chart included between $-32°$ and the south pole, entrusted to the observatories at the Cape, at Melbourne, Sydney, and Perth (western Australia).

Influenced by these considerations, Sir David Gill has already determined the positions of 8,556 comparison stars of his zone by very careful meridian observation—about twelve for a plate and three meridian observations at least for each star.

To fulfil the requirements for the chart of the heavens relative to this region, it would be necessary to obtain the accurate positions of about 30,000 stars, which would give about twelve comparison stars for each plate of four square degrees.

Instead of observing in an expeditious way, according to the methods recommended by the Astronomische Gesellschaft, 60,000 stars, requiring, according to your estimate, about 150,000 observations, I would prefer that the positions of 30,000 only should be determined with greater accuracy. This would possess the double advantage of giving, on the one hand, a very precise basis for the photographic catalogue of this part of the sky, and, on the other hand, of providing a solid foundation for determination of the proper motion of the stars. It is very desirable that we should have at our disposal, at an early day, the results for this element, which is of such fundamental importance in modern astronomy. With this object in view, to make observations with twice the degree of accuracy is to reduce by one half the interval of time necessary to render sensible the proper motions of the stars.

It would also be desirable, in order to place the catalogue on a sure and homogeneous foundation, to combine it with the 6,000 stars above the seventh magnitude which you propose to determine with the most rigorous precision. A catalogue constructed under these conditions would certainly serve in the future as the starting point for all researches of high precision concerning the study of proper motions between —32° and —90°.

Before leaving this subject I will present another example to illustrate its great value. In addition to the plates of short exposure intended for the construction of the Astrographic Chart, comprising all stars down to the eleventh magnitude, it is also intended at the same time to secure plates of long exposure, guarded by the same precautions and including stellar images down to the fourteenth magnitude. Then, if necessary, the plates of this second series could be measured and reduced with the same degree of precision as the first. Since a great many of the plates contain the images of 1,000 to 12,000 stars, one can readily see that, after photographing for a second time one of these regions so rich in stars, the comparison would reveal a very large number of proper motions for all grades of magnitude down to the fourteenth, a feature of this plan which might prove very instructive from the cosmogonic point of view.

In your program you point out the necessity of determining the parallax of stars; the value of such work is incontestable.

In my opinion the surest method and the one least subject to systematic error is the photographic method properly applied. It would therefore be very judicious to provide for this purpose a

photographic instrument similar to that used for the Astrographic Chart. This instrument could also render valuable service in other lines of work.

Among the eighteen observatories that were associated in the work of the Astrographic Chart there is one that has not held to its engagement. This is the observatory at Santiago, Chile. It is in order to fill its place. You could furnish invaluable assistance to this vast scientific enterprise should you also devote this instrument to the work of the Astrographic Chart.

In a favorable climate an experienced observer, aided by two assistants, could accomplish it in about eight years and could secure all the plates for this zone—1,260 plates for the catalogue and 1,260 for the chart. This period of time should suffice, now that all the methods are so well established.

In providing the catalogue of comparison stars, or by undertaking the vacant zone, or, better still, by accomplishing both tasks at the same time, America would find her opportunity to occupy a most honorable place in this vast international enterprise, whose success, which is now assured, will be without the least doubt the occasion of numerous and noteworthy discoveries.

Finally, it seems to me proper to mention a very useful and inexpensive work which could be undertaken by one or the other of the two proposed observatories, and which up to the present time has been wholly neglected in the southern hemisphere—the systematic observation of meteors. An experienced astronomer, with the aid of one assistant, in three or four years would be able to produce important results in this line. It is in respect to the discovery of stationary radiants, the activity of which lasts for weeks and even months, that he would supply most valuable material for the study of the causes and conditions which produce this curious phenomenon. On the other hand, one could also reasonably hope to run across some notable swarm of meteors in connection with the periodic comets, examples of which, still so rare (four in all) have been observed only in the northern hemisphere. This would be a particularly desirable contribution, which might render possible a more searching investigation of the close relation existing between comets and meteors; also comparison of the frequency with which meteors appear in different seasons of the year cannot fail to bring enlightenment on the origin and velocities of meteors.

In that which concerns the location of the astronomical station the latitude should certainly not be north of —30°, as you have

indicated. It would be advisable at the same time to seek for a climate as free as possible from dust and cloudiness. Perhaps a desirable location could be found in the Argentine Republic, preferably a little south of Cordoba. The climate of that city seems to have lost in quality during the last few years; the winds filled with dust have become more frequent.

[*From Director H. Bruns, of the Royal Observatory, Leipsic.*]

[Translation.]

LEIPSIC, *July 23, 1903.*

In answering the questions proposed by you, permit me first to formulate the following propositions: Astronomy, now and in the future, is confronted with the task of attaining in reference to the universe of stars what has already been accomplished for the solar system, namely, a sure knowledge of the space arrangements and of the motions of the bodies therein contained. Furthermore, the duty is imposed upon each generation of astronomers to contribute toward the attainment of that end whatever is possible with existing means.

If, now, one reviews what has been already accomplished in this field, it will be seen that the weak point is not the extent and quality of available observations, but the unequal distribution of them upon the two halves south and north of the equator. From this inequality arises a sensible defect in all discussions which have for their subject, not single objects or groups of objects, but the heavens as a whole. Therefore, without any reservation whatever, I agree with you that an attempt should be made to lessen this defect, especially since an effective remedy through increase in the permanent observatories of the southern hemisphere is not otherwise to be expected at present.

As to that which concerns the individual propositions set forth by you, I am of opinion that the strengthening of the weight of exact star positions has precedence over all other things. Observations for parallax, velocity in the line of sight, etc., can be secured at any time, whenever the means are at hand, without fear that a marked disadvantage to the development of astronomy would result from such delay. On the other hand, there is one thing that cannot be retrieved through later observations, and that is the "epoch" of a catalogue.

Of the catalogue works mentioned in your communication, the extension of the zones of the Astronomische Gesellschaft to the

south pole is, in my eyes, the one most important for the future of astronomy. This assumes in advance, however, that the work designated by you as No. 1, for the brighter stars, should precede. Therefore, I entertain no doubt that proposition No. 1 should have the preference over all the other tasks.

[*From Director Küstner, of the Royal Observatory, Bonn.*]

[Translation.]

BONN, *July 27, 1903.*

I have read your letter of July 8 and the enclosed program of the Committee of the Carnegie Institution with the greatest interest, and I hasten to express to you my full and unqualified acquiescence in the propositions therein contained.

The situation is so clear and simple that, in my view, only one answer is possible to the question, "How can astronomy be promoted to the best advantage?" namely, through the establishment of an observatory equipped with the best instruments in the most favorable location in the southern hemisphere. The present neglect of the southern sky is felt in the most troublesome manner in all astronomical problems, and many series of observations that have been secured in the northern sky with great care and at great expense cannot be fully employed for the benefit of science because they pertain to only a part of the sphere. The most important conclusions can be reached only after these have been equally extended over the southern sky, and then only will the finest fruits of astronomical investigation begin to ripen.

I can but join in approval of the list of works, arranged in a preliminary way according to their importance, which is proposed for this Southern Observatory. This list might easily be further increased, but practically, at the outset, it may have to be curtailed.

I hold point 1 [meridian observation of stars down to the seventh magnitude] as the most important and—because closely related to it—point 4 [observation of all stars down to the ninth magnitude, southward from —32°]. The prompt continuation to the south pole of the great undertaking of the Astronomische Gesellschaft, which has already been extended down to —23° or —32° is an unconditional necessity. This last quarter of the sky must soon be worked out, if we are not to lose a considerable part of what has been accomplished in the three quarters already completed.

In the continuation of this plan of the Astronomische Gesellschaft

one will naturally profit by the experience which has been gained in the meantime. For example, the personal equation for magnitude should not be simply determined in an incidental way, but directly eliminated. Further, the zero-stars should be observed not merely at the beginning and end of the zones, but they should be regularly distributed throughout the zones, in such a manner that they should be observed at average intervals of about ten minutes.

For the next most important works I consider points 2 and 3 to have the preference [point 2, determination of stellar parallax; point 3, determination of the velocity of stars in the line of sight].

As to the question of choice of a suitable site, I am unfortunately unable to make any definite recommendation. The precision, and consequently the value, of astronomical observations is dependent in so large a measure upon the condition of the atmosphere, that, in the choice of site, regard for the best possible conditions of atmosphere must be the controlling factor, especially when an observing station is to be established for a few years only. If the question turns on the establishment of a permanent observatory, then, by all means, proximity to some center of civilization is also to be considered.

Furthermore, I can but agree in the most complete conviction with the proposition for a great astrophysical observatory especially for solar investigation. The only scientifically correct and at the same time practical way to attain the beginnings of knowledge as to the nature of the fixed stars is, in the first place, through most exact investigation of the star nearest to us, the sun, concerning whose constitution so many obscure problems prevail.

I hope that the Committee may be successful in making the large resources of the Carnegie Institution useful for astronomical investigation in the manner proposed.

[*From Director H. Seeliger, of the Royal Observatory, Munich, President of the Astronomische Gesellschaft.*]

[Translation.]

MUNICH, *August 3, 1903.*

The establishment of a new observatory in the southern hemisphere, with a large provision for instruments, must be regarded as a desideratum for astronomy. The opinions of astronomers will not differ in this respect, and it would be quite unnecessary to prove this in detail, especially since the Statement which you have been good

enough to send me handles the question completely to the point, and exhaustively. The problems that fall to such an observatory arise through the development of modern astronomy, and, indeed, in this Statement they are so fully and clearly enumerated that very little can be added. The main point is to establish the requisite balance between observations made upon the northern and the southern hemisphere, which, hitherto, the few southern observatories could not possibly maintain. Any arrangement, according to their importance, of the classes of observations that ought to be made will naturally depend upon the standpoint of the one who renders judgment, and therefore I can only say that I hold the order given in the Statement to be essentially sound. On the other hand, one can make good the claim that the astronomer has not only to collect the knowledge which will enable later generations to derive important results, but also that he should especially challenge those problems that will permit general conclusions to be drawn within a time not distant, even though in a fragmentary way only.

From this point of view I have for several years expressed the opinion that, though observations of fixed stars made to establish the *motions* of the heavenly bodies are important beyond doubt (though they will first bear fruit in the distant future), yet observations concerning the *present aspect* of the starry heavens should not be neglected. This aspect has an independent interest of its own, and from it a valuable result can be drawn at once without waiting for the cooperation of future generations. Accordingly, on account of their bearing upon my own investigations in regard to the space relations of the stellar system, I would like to designate as especially important the following problems for which the cooperation of the observatories in the southern hemisphere is absolutely necessary, since it is possible to make these discussions only upon the basis of observations distributed over the *entire* sky:

(1) Determination of parallaxes, which appears as (2) in the Statement.

(2) An investigation of the apparent distribution of the stars in the southern hemisphere, including those of the faintest magnitude that can be observed. The Statement excludes the consideration of new problems, to be sure; but here we are concerned, not with a new work, but with an old one which has not hitherto received sufficient attention, the object of which is to solve the new problems more conclusively and the execution of which would, in effect, establish the solutions upon a sounder basis. A similar work is now

being carried out for the northern hemisphere at the Munich Observatory, though in a very modest way, since the means at its disposal are very limited, and in this connection I would refer to my report in the Vierteljahrschrift of the Astronomische Gesellschaft.

(3) Very many—indeed, most—of the investigations in stellar astronomy depend upon the establishment of an exact photometric scale of magnitudes. For the northern sky, in this respect, the magnitudes of the stars down to about the eighth magnitude are fairly well established through the labors at Cambridge (Mass.) and Potsdam. In this connection is to be noted the demand for fixing by photometric methods the magnitudes of the fainter stars, through a satisfactory choice of objects, evenly distributed, down to the faintest which can be observed. This is still an object to be desired. For the brighter stars in the southern hemisphere an extensive series of observations is available. But undoubtedly this is not free from objection, and we are not in position to establish the distribution of the stars upon the southern sky in combination with that upon the northern hemisphere.

The Statement does not propose to take up photometric and photographic works for the present at least. But I think that the works designated under (2) and (3) are those that are now most pressing and, at the same time, those that would be most acceptable. Here, within a few years, if sufficient means exist, we may hope for results that will be of the greatest importance for all time. Of course, one would better observe with the same instrument first at a station in the northern hemisphere, and then from a station in the southern hemisphere, or one would have to choose a place in the neighborhood of the equator, and from that point survey the entire sky, which, of course, would not be entirely free from objection.

The choice of a place for an observatory I consider to be extremely important. When one reflects what limitations and disadvantages the climate in our latitudes impose upon all astronomical observations, one can only look with envy upon the astronomer who is permitted to live in a good climate. What a mass of provocations and waste of time is such an astronomer spared! I take this opportunity to call your attention to the Australian continent, or to Tasmania, whose wonderful and, at the same time, healthy climate I know from my own experience, though from no more than a residence of a few months.

If, in conclusion, I may present an arrangement of the works mentioned in the Statement, according to my opinion of their im-

portance in relation to the views I have expressed in the foregoing, I would arrange them thus:

1. Point (2) of the Statement (parallaxes).
2. Point (3) of the Statement (radial motions).
3. Point (1) of the Statement (meridian observations).
4. Point (4) of the Statement (zone observations).

Your further arrangement corresponds perfectly to my views.

At all events, a great and inestimable gain for astronomy would be realized if your views in reference to a new Southern Observatory should come to fulfillment, and it appears to me that in that event it is not so much a question *what* important works shall be assigned to the new observatory, but rather that there are in general important works which it has to accomplish, and that all in the Statement are such I am fully convinced.

[*From Sir David Gill, Astronomer Royal at the Cape of Good Hope.*]

CAPE OF GOOD HOPE, *12th August, 1903.*

You ask on behalf of your Committee my views on the subject of the most urgent needs of astronomy.

There cannot be the slightest doubt that from the highest standpoint what is most urgently required is an increase in the astronomical equipment of the southern as compared with the northern hemisphere, and this is equally true in the departments both of the older astronomy (astrometry) and astrophysics.

There are urgent needs in both of these departments. The relative urgency will vary, in the opinion of many, according as the individual's knowledge or sympathy lies with one department or the other.

Astrometry.

A, 1. In connection with the older astronomy, I entirely concur that the establishment of an additional meridian circle of the very first class in the southern hemisphere in an ideal observatory for fundamental observations is a first essential. Practically the Cape is the only observatory where really fundamental work is being undertaken, and some independent check or comparison is necessary if only to give assurance of the accuracy of results arrived at.

It may be remarked that most of the observatories of the northern hemisphere are defective in the form of covering or observatory for their transit circles and in the means of equalizing the internal with

the external temperature. Therefore, if any transit circle is to be sent to the southern hemisphere, and is to be used before or afterward in the northern hemisphere for the purpose of comparing results, it seould be provided, for both series of observations, with a modern steel observatory having double or triple walls, means of conveying the convection currents away from the observing opening, and of separating the whole observatory into two halves, so that the instrument may be used as nearly as possible in the open air.

There is a still more important condition which should also be fulfilled, viz., provision of means to avoid personal equations depending on magnitude or upon the velocity of the star's motion (*i. e.*, the star's declination). It seems to me that the only system of observing in R. A. which permits this possibility is the Repsold-Struve method, in which a wire is made to travel across the field at nearly the same velocity as the star. The eye piece travels with the wire, so that, if the mechanical conditions are properly realized, the observer, having bisected the star disc with the wire, should view the disc so bisected apparently as if at rest, and be able by simple means to correct any errors of this bisection which he may notice during transit and which may be due to errors of the clockwork, the driving screw, or the original pointing. The drumhead of the screw which causes the slide of the moving wire to travel is provided with contacts which, as the drum rotates, make electric contact with the chronograph circuit, and so record the instants when the pointing on the star would correspond with the particular readings of the micrometer head.

This method, so far as I am aware, is the only one not liable to personality depending on magnitude or declination, and this, although we have not yet absolute proof, we believe also to be free from personality in observations of the sun and moon.

The necessity for provision of reliable azimuth marks and of a clock not liable to diurnal variation of rate is too well known to require further reference.

A separate memorandum dealing with some details of the abovementioned methods will be forwarded.

As to a site, I think it would be difficult to find in the southern hemisphere a better one for this purpose than the neighborhood of Bloemfontein. I venture to think that in connection with this plan one or two of the northern observatories should be provided with a better form of observatory or covering for the transit circle.

A, 2. There are two other problems of the older astronomy which cry out for solution, one of which is a comparatively small affair, the other a very big one, but both are urgent. I refer to the completion of the organization in the southern hemisphere for determining change of latitude (that is the smaller affair), and to the formation of a parallax Durchmusterung—*i. e.*, determining the parallaxes of *all* the stars to a certain order of magnitude—(that is the large one).

I take the simpler matter first. Mr. Chandler proposes a southern belt of observatories:

	Lat.	Long.
Sydney	$-33°\ 51'$	$-151°\ 2'$
Cape of Good Hope	$-33\ \ 56$	$-\ 18\ \ 5$
30 miles south of Santiago	$-33\ \ 54$	$+\ 70\ \ 7$

It should involve very little trouble and comparatively small expense to establish the necessary organization at Sydney and the Cape, and I venture to think that the Carnegie Institution could not be better advised than to provide at once for the observatory near Santiago, equipped with two observers devoted wholly and solely to determination of the aberration constant and change of latitude. The instrument I would recommend for all the three observatories would be the photographic almucantar and the method used by Cookson (see Monthly Notices, R. A. S., LXI, p. 315). There should be observations of every group of stars in the early evening, the early morning, and near midnight, at all times when opportunities occur. In this way we ought to get an extremely accurate determination of all the latitude changes and a powerful determination also of the aberration constant.

A, 3. The parallax Durchmusterung is a very much greater undertaking, but it is of the supremest interest to science. I do not think it desirable to go beyond magnitude 9½ or perhaps 9; even then the taking and measuring of the plates is a very big business and involves a large organization.

A telescope of large aperture is not necessary, but considerable focal length is requisite to give the necessary precision of measurement. The highest optical perfection should be arrived at, probably a 4-glass objective of 8 or 10 inches aperture and 20 feet focus would be most suitable. I think it very undesirable to employ a cœlostat or any plan involving reflection from a plane mirror, as plane mirrors may be liable to flexure or deformation by temperature changes.

Kapteyn's method should be adopted, and all the photographs taken at different seasons on the same plate should be exposed at the same hour angle. Although this involves some sacrifice of parallax factor it eliminates many possible sources of systematic error. I would advocate an observatory (that is to say, a telescope and two photographic observers) devoted exclusively to this work, with an office and staff of measurers and computers located elsewhere, where the services of students and others could be secured, living is less expensive, and facilities for instrumental construction and repair are more accessible than in the southern hemisphere. To complete the work there must of necessity be a corresponding observatory in the northern hemisphere, and to complete the whole work in any reasonable time there should be several such pairs of observatories.

To make a beginning, so as to test the accuracy and probable value of the work thoroughly, it would be well to install one observatory of the kind in the most favorable situation and to confine the work, say, to four overlaping areas at each alternate hour of R. A. in each zone of 4° in declination and from declination 60° at each 4 hours of R. A. The results of such a series of pictures, taken and discussed, would lead to results of immense general importance, and would give some close approximation to the average parallax of stars of different magnitudes and proper motions, and would be an excellent pioneer program to ascertain the weak points of the original arrangements. It would not too greatly increase the program if plates having for their centers a number of the stars of more remarkable proper motion were added—indeed, perhaps the program might be best begun with these.

A, 4. I am hardly disposed to support the plan suggested by you of extending the zones on the plan of the *Astronomische Gesellschaft* from declination —32° to the south pole.

It is a far more accurate and useful plan to select the stars which are best distributed for determining the constants of photographic plates, as has been done at the Cape for declination —40° to —52°, and then to determine from the photographic plates the places of all required stars—say all stars to the eighth or ninth magnitude.

On the *Astronomische Gesellschaft* plan you get for this purpose an unnecessary number of stars in some parts of the sky and an insufficient number in others. For the zone —40° to —52° I found 8,000 stars ample, and they are as uniformly distributed as it is possible to select them. From the coördinates of our plates (and we are

about to publish zones —40° to —42°, the rest following in course of a few years) with the plate constants which will also be published any computer can construct a catalogue of all stars to the seventh, eighth, ninth, tenth, or eleventh magnitude, as he may see fit.

A, 5. The discovery and measurement of close double stars is an important branch of astrometry which is far behind in the southern hemisphere. I have great hopes that ere long a powerful telescope will be erected at Johannesburg for this purpose. Mr. Innes, recently my secretary, has been appointed in charge of the observatory there. At present his work is officially that of meteorologist, but I have great hopes that, having regard to his proficiency as a double-star observer, his enthusiasm and his power of exciting scientific sympathy, and the number of wealthy and large minded residents there, he will ere long be provided with a first class equatorial fitted for research on double stars. Meanwhile I propose to lend him the portable observatory I used at Ascension and a 6-inch equatorial of my own, which he is to employ in making an independent determination of the magnitudes of a number of stars on each of the C. P. D. plates.

But this prospect should in no way interfere with the erection of a second large telescope devoted to the same work, for independent comparison is at least as important in this department as in fundamental meridian work.

I had the satisfaction, when I visited Johannesburg last May, of selecting a site, outside of the town, which I have little hesitation in saying is one of the finest in the world for an observatory. It is nearly 6,000 feet above sea level, the atmospheric conditions seem to be most favorable, and on my recommendation the site, 10 acres in extent, has been secured by the government.

So much for astrometry. I agree with you that the provision made under the direction of Pickering at Arequipa and the labors of Roberts and Innes in South Africa sufficiently fulfill the requirements of photometric research as compared with that class of work in the northern hemisphere, and the work of the Carte du Ciel appears to be provided for.

Astrophysics.

B, 1. I have no hesitation in saying that what is required is the erection of the largest possible reflecting telescope for exact researches on the spectra and motion in the line of sight of the fainter

stars. What you want is an instrument that will collect the largest possible amount of light from a star within the jaws of a spectroscope slit.

Not only does a reflector provide this on a larger scale than it is possible to attain in a refractor, but it unites the rays of every refrangibility in one focus.

For spectroscopic work a Cassgrain reflector seems the best form to adopt, as the cone of rays, with its smaller angle of convergence to focus, permits use of a comparatively long collimator. The position of the spectroscope is also convenient, because, having regard to the weight of the speculum, the radius of motion of a spectroscope near the speculum end must be much smaller than that of one attached near the principal focus of the large mirror, and it is also much more convenient of access in the former than in the latter case.

Of course, photographs of nebulæ, &c., could be taken in the focus of the principal mirror, but this is a less urgent need than the spectroscopic researches.

With the modern ball bearings and electric-motor motions I see no difficulty in conveniently mounting a mirror of 6 or 8 feet diameter.

The erection of such an instrument in some very favorable position is, I think, the next great step that should be taken. We are in a position now, with refined and well studied apparatus, to attack the determination of motions in the line of sight of all the brighter stars ; but to get sufficient light to photograph the spectra of the fainter stars under such dispersion as will furnish reliable determination of motion in the line of sight requires a telescope of greater light grasp than I fear we shall ever get from a refracting telescope. For less money than the cost of a 40-inch refractor one could mount a reflecting telescope of twice that aperture—*i. e.*, of four times the light grasp—that would unite all the rays of light from a star in one focus.

I put this so far beyond all other demands of astrophysics that I make no further suggestion, and I do feel that steps should be taken to urge its fulfilment.

I believe that Bloemfontein, in the Orange River Colony, would be an ideal site for the erection of such an instrument.

[*From Dr. M. Nyrén, Wirkl. Staatsrath, Pulkova Observatory.*]

[Translation.]

PULKOVA, *August 14, 1903.*

The proposition for the observation of stars in the southern hemisphere, which you were so kind as to send me, interests me greatly; so much the more in reference to some points of the program, because we also here have for a long time cherished ideas tending in the same direction. As a small contribution to the realization of these ideas may be considered, the extension of the Pulkova Fundamental Catalogue to —30°, now attained by observations in Odessa. By numerous reference stars these observations are intimately joined with the Pulkova system. I send by this mail the program and the list of stars for this combined catalogue.

I quite agree with you that the first task for the observing station in the southern hemisphere should be to create a trustworthy fundamental catalogue from —20° to the south pole. In this catalogue the Auwers list of 480 stars, extended to the pole, should be included; but I would add thereto a number of stars, especially in those places where the stars of Auwers' catalogue seem to be brighter than may be desirable for fundamental stars, third magnitude and brighter. In this manner we should obtain a standard list of about 600 stars, in accordance with your proposition.

For the observation of the right ascension of these stars it would not be troublesome to find many places qualified for the purpose. With the declination it is doubtless more difficult; in this regard even the most southern portions of Australia are, as I think, too far from the poles. Besides, the observations made at the most southern observatory, Melbourne, are apparently affected by suspicious anomalies in refraction. In my judgment, the fundamental declinations should be observed on the continent of South America, as far as possible south of the —40° parallel, at a station where the ground to south and to north is as nearly similar as possible. Under favorable atmospheric conditions two observers could accomplish in two years a fundamental catalogue of 600 stars with 8 to 10 observations of each. For the great secondary catalogues, which demand a longer time, the observations, in case of need, could be made at another station, and also with another instrument.

As the number of fundamental determinations of the southern stars is rather small, the catalogue in question should necessarily

receive great weight. It is then, according to my opinion, the best economy of time and money to provide the observing station with equipment of the first rank. Your proposition that the same instrument should be employed alternately north and south of the equator will help us to eliminate many errors from the star places. Concerning the nature of the instrument, I should, for the declinations, in place of the meridian circle, prefer a vertical circle of moderate dimensions—5-inch aperture. The vertical circle seems to me, for a new observing station, preferable also from this point of view, that the consolidation of the instrumental piers, which for a meridian circle requires a long time, is for the vertical circle of no importance.

As corresponding in accuracy to the declinations determined in such manner, the right ascensions should also be observed with a transit instrument. This part of the work could without inconvenience be made at another station, at Cordoba, or in Australia.

For the observations of the stars of the secondary catalogues, the instrument described by you will certainly do good service.

I submit these, my ideas, to your judgment, and I should be glad if you find them worthy of any attention.

I have delayed the answer of your letter until Dr. Backlund's return from a journey in Germany.

[*From Director O. Backlund, of the Imperial Observatory, Pulkova.*]

[Translation.]

PULKOVA, *August 20, 1903.*

In order to make my answer to your valued communication more intelligible, I premise the following remarks:

Fundamental determinations of star positions stand in the first rank among the chief undertakings of the Pulkova Observatory. To this end W. Struve had the transit and vertical circle constructed, by means of which the positions of the so called Pulkova "Hauptsterne," 381 in number, have been determined in three series, namely, at the epochs of 1845, 1865, and 1885 (about). In the year 1894, in accordance with a plan by Nyrén, the program of the two instruments was enlarged by Bredichin so that about 1,000 additional stars of the fourth to seventh magnitudes should be determined for the epoch 1900, in general after the same program as for the "Hauptsterne." This series of observations is now completed. When I undertook the directorship, in

1895, I endeavored still further to extend the fundamental observations, and accordingly brought about the establishment of a branch observatory at Odessa (north latitude 46°) whose task it is to extend the limits of our fundamental observations to —30° of declination. In accordance with the experience at Pulkova during the course of sixty years, the transit and vertical circle were selected as the instruments best suited for making these observations. After thorough reflection I decided in favor of dimensions of these instruments smaller than those of the Pulkova instruments. Since stars fainter than seventh magnitude would not be observed, the aperture of both instruments was fixed at 4 inches and the focal length at 4½ feet. With dimensions so small the observations can be made far more conveniently, and with the vertical circle twice as rapidly as with the Pulkova instrument. The observations for a fundamental catalogue of the same extent as that of the Catalogue of the Pulkova "Hauptsterne" were begun on the third of February, 1901, and, so far as the number of observations is concerned, upon the same program as that of Pulkova. The work was completed exactly a year after beginning, while such a series of observations at Pulkova has never been completed hitherto within less than seven years. Of course, this rapidity of work was made possible not alone on account of the more convenient instrument, but also on account of the decidedly better climatic conditions. The observations are already reduced, and it proves that their accuracy comes up to that attained at Pulkova. Both at Pulkova and at Odessa Repsold's self registering micrometer is adopted. This autumn the observation of the Pulkova "Hauptsterne" is to be taken up for the fourth time, and for the epoch 1905. On this occasion the number of stars has been increased to 500. Simultaneously about 200 more southerly stars will be observed at Odessa, so that the resulting catalogue will contain about 700 stars independently determined between the North Pole and —30° of declination.

It will probably be clear, after this circumstantial description of our fundamental observations, that nowhere can the news of the purpose to institute fundamental observations of about 700 fundamental stars in the southern hemisphere awaken a higher interest than here in Pulkova. Indeed we are permitted to hope, in accordance with your grand project, that at no distant time the long desired and absolutely necessary fundamental observations will be extended over the entire sky, to aid in the further development of

our knowledge of motions in the planetary system and in the stellar field.

That the observations, especially those which concern the declinations, should be made, if possible, under a southerly latitude of at least —40°, I am in agreement with Nyrén. As to that which pertains to the planning of instruments, in consideration of the high degree of technical skill in the construction of instruments in America, it might appear venturesome to express decided opinions from here. I content myself with calling attention to the experience which we have gained here in Pulkova and in Odessa with the transit and vertical circle as to the determination of right ascension and declination with special instruments.

If, in addition to the fundamental determinations, the projected zone observations can also be secured, you will in that way earn the thanks of astronomers for all time.

There can be no doubt that the derivation of astronomical constants from observations in the southern hemisphere is a thing to be desired. For example, the very discordant values of the aberration constant, which the determinations secured at different observatories have recently shown, prove how necessary classic observations are. Latitude variations demand observations at places separated from one another as widely as possible. In accordance with an agreement with Potsdam, a series of observations for this purpose will be undertaken here in Pulkova, and with a zenith telescope of 5 inches aperture.

In regard to your remaining questions, so far as I may be permitted to judge, they are not less rationally proposed. You will be able to count upon the unanimous approval of the scientific world not less for these than for the others.

That the existing arrears in astronomical work in the southern hemisphere is felt as a drag at every step in every investigation now in progress cannot be denied. If on that account the investigation planned by you is realized, then a great drawback will be thereby removed; but the well known American energy affords a warrant that the carrying out of this plan will not be long delayed. A speedy realization of this project means an immense advance in science.

[*From Director E. Becker, of the University Observatory, Strassburg.*]

[Translation.]

STRASSBURG, *August 23, 1903.*

I take note of your letter of July 8 with the highest interest, and I fully assent to the opinions therein expressed. It is a fact not to be overlooked, and one originating in the distribution of observatories upon the earth, that our knowledge of the southern heavens is very much behind that of the northern hemisphere. So long as this inequality exists we must necessarily relinquish the idea of obtaining a satisfactory solution of many and indeed the most important cosmic problems. That this deficiency will be remedied within a time which we can now foresee by the establishment of new governmental observatories in the southern hemisphere is not to be expected, and an effort must be made—which would be greeted with the greatest joy in case of success—to induce private institutions to lend a helping hand. An observatory established in a favorable site, equipped with modern instruments, under skilled and energetic direction, with a staff of experienced observers and practiced computers not too small in number, in my judgment would be able to complete a work which would suffice to fill up the gaps in our knowledge that are now most deplored.

What tasks are deserving of the most prominent place on the program is a question which it is not entirely easy to answer from an objective point of view, and the answer would also depend upon the means that are available. On the whole, I am in sympathy with the arrangement set forth in your statement, and, for my part, would assign the preference to propositions 1, 3, 4, and 5, without underestimating the importance of the other tasks. As to 1, it appears to me worthy of consideration, whether upon grounds of economy, the undertaking ought not to be limited to stars of the sixth magnitude, or, in any case, to those of the sixth and one half, and whether the determination of position for stars fainter than the sixth or sixth and one half magnitude should not be assigned to the zone work, which, according to experience, is susceptible of producing very accurate results.

[*From Dr. Ralph Copeland, Astronomer Royal for Scotland.*]

EDINBURGH, *26th August, 1903.*

I have read your letter and inclosure of July 8 with deep interest. Regarding the various classes of special observations to be made in the southern hemisphere, included in your statement, it seems to me that classes 1, 4, and 3 are of the greatest immediate importance, and their urgency is probably in the above sequence, the first being the most important.

Class 2 I consider of relatively far less immediate interest, seeing that the results arrived at are by no means of the fundamental character of those obtained by classes 1, 4, and 3.

Respecting class 1, it is conceivable that the observations might advantageously be divided into two groups: *a*, the fundamental determinations of the places of 600 principal stars, together with the essential observations of the sun; *b*, the precise observation of the 5,400 remaining stars brighter than the seventh magnitude between —20° and the south pole.

Group a.—Possibly this work could be most satisfactorily accomplished by using two instruments—a vertical circle and a specially efficient transit instrument. But these instruments, and in particular the cells of the object glasses, should be made of mild steel, which has a coefficient of expansion differing but little from that of glass and much smaller than that of brass, hitherto so largely used in the construction of astronomical instruments of precision. With the lenses held by springs on the plan designed by Fraunhofer, it is probable that they would rest almost absolutely immovable in their cells in all positions of the instrument, and that their minute real movements would be directly related to changes of temperature.

For the vertical circle I would suggest the trial of a design that occurred simultaneously to the late Dr. Common and to myself. It consists in placing an object glass at each end of the tube in such manner that the focus of either object glass shall fall absolutely on the outer surface of the other. Spiders' webs are to be replaced by fine lines, engraved or etched on the outer surface of each object-glass (I have seen lines of this kind not appreciably inferior to the finest natural webs). This construction would permit of the "end for end" reversal of the vertical circle, as well as of the ordinary "right and left" reversal, thus eliminating flexure from every determination of double zenith distance. I pass over the obvious

details of the light swing-frames with their counterweights carrying the eye piece at each end of the telescope and readily turned aside when not in use. I feel assured that such an instrument in the hands of a skillful observer would yield zenith distances of an accuracy not yet attained. One of our first opticians assured me that by the use of twin discs of glass and alternate grinding there would be no difficulty in producing the requisite pair of object glasses.

The transit instrument should be reversible on every object. It should have a clean drawn cylindrical tube of mild steel, attached in the simplest efficient manner to the middle of the enlarged steel axis through which it passes. This kind of tube was suggested by Sir David Gill in conversation many years ago. The transit to be taken by means of a movable recording wire—say, 10 seconds in each position of the instrument—by the well known method used so largely by Professor Albrecht and his staff in Germany. By this method collimation error and inequality of pivots are at once eliminated, and, as it seems to me, the troublesome magnitude equation is practically evaded. Moreover, it is certain that the personal equation is confined within extremely narrow limits. As it would be necessary to observe the sun with this instrument, it would probably be desirable to use a reversing eye piece; but experience would doubtless soon show whether this is desirable or not.

Group b.—This work—the precise observation of the 5,400 remaining stars brighter than the seventh magnitude between $-20°$ and the south pole—could probably be rapidly and efficiently accomplished by the use of a meridian circle, which I should like to see made of steel, with the graduations on gold, platinum, or on an alloy of gold and palladium; but by no means upon silver, which is so liable to tarnish and necessitate risky cleaning.

The observations in class 4 are well worth undertaking with the least practicable delay, as they will gain largely in value by every year that elapses after they are once secured. By carefully boxing in the circle of the instrument used and securing an efficient circulation of the air confined within the box, it is probable that the accuracy of this class of observations could be measurably increased. Class 2 seems to me to be the least important part of the proposed undertakings, the resulting stellar distances being apparently peculiarly mixed up with those of the few available comparison stars. But may we not hope, now that the displacement of the earth's axis of rotation with regard to the observatory can be taken into account, that the fundamental observations of class 5 may begin to indicate

the annual parallactic displacements of all the nearer stars relative to the bulk of the 600 stars under observation? In course of time it would then be necessary to take these parallaxes into account in determining the apparent places of the stars affected by them.

Class 6 evidently calls urgently for a large refractor of the most perfect kind, used at a station where the definition is of the very best. Possibly this work could be best carried on at the proposed high level astrophysical observatory, although I may mention that at Jamaica, in the end of 1882, I found the definition near the level of the sea exceedingly good on many nights. In that island it would be possible to find a perfectly salubrious station at an elevation of 4,000 to 5,000 feet, and quite probably equally favorable localities could be found in some of the more mountainous South Sea islands.

The general sidereal part of your scheme ought, as you say, to be carried out south of latitude —30°, but in the southern hemisphere the climate increases in severity at relatively moderate distances from the tropics much more rapidly than it does north of the equator; hence every care should be taken to avoid a site too far to the south. At the same time I most earnestly support your view that no pains should be spared in choosing a thoroughly salubrious climate; otherwise the most devoted members of the expedition will be just the most likely to fall victims to any error of judgment in this most important matter.

In conclusion, I would suggest placing your most important timekeeper in a partial vacuum—say, under nine tenths of the normal local atmospheric pressure. This is easily secured in a cast iron box with a three-quarter-inch glass face resting on a slip of rubber. A "quicksilver sleeve" permits of winding the clock twice in the week. A small syringe removes any slight leakage of air. We have found here that the air in the box must not be dried artificially, or the oil necessary to the clock work will decompose and the clock will stop.

A great improvement would be to add an outer case, the air in which, by a simple electric contrivance, could be kept at a uniform temperature slightly in excess of the highest temperature likely to occur naturally. Under these conditions, any well made clock ought to have a rate subject only to very minute changes.

[*From Dr. W. H. M. Christie, Astronomer Royal, Greenwich.*]

GREENWICH, *August 29, 1903.*

I fully agree with the Committee that a much larger provision for astronomical observations in the southern hemisphere at the present time is desirable, and that with this object a Southern Observatory of an expeditionary character for definite pieces of work which could be completed within a limited number of years should, if practicable, be established.

Taking the special observations regarded as important by the Committee:

1. The proposed fundamental determination of star positions would be of great value. With this should be combined observations of the sun and determination of position of the ecliptic.

Observations of the moon, as nearly continuous as possible *throughout the lunation*, for several years are very much needed for the improvement of the lunar tables in regard to terms of short period, and these might well be combined with the observations of fundamental stars.

Observations of the planet Mercury are also much wanted.

2 and 3. I fully agree as to the importance of these and the need for making provision for them.

4. This does not seem to me so much needed at the present time. The observation of reference stars for the plates of the astrographic catalogue now being carried out at Cordoba, the Cape, Sydney, and Melbourne largely covers the ground, and though the number of stars is less than on the plan of the *Astronomische Gessellschaft*, the place of a far greater number will be determined from the photographic plates with an accuracy greater than that of meridian observations.

There is, however, a gap in the southern zones for the astrographic catalogue, viz., zones —32° to —40°, the plates for which are being taken at the Perth Observatory (West Australia), but with little prospect of their being measured there or of the necessary reference stars being observed. Another zone, —17° to —32°, undertaken by Montevideo, is in even worse condition, the funds for providing the photographic telescope not having been granted as yet, though promised by the president of Uruguay.

In place of 4, I should prefer to substitute the completion of the zones of the astrographic catalogue by the taking and measurement of the plates and the observation of the reference stars for the zones

not otherwise provided for. This seems to me to be urgently needed, and the work could be completed in a limited number of years.

5 and 6. I quite agree as to the importance of these.

7, I presume, may be considered later.

In the matter of location, I would submit that New Zealand and Tasmania should be carefully considered. An observatory established in New Zealand would have a good chance of being taken up by the government, as in the case of Cordoba, after its specific pieces of work were completed, and it would undoubtedly give a great stimulus to astronomy in the colony.

[*From Professor J. C. Kapteyn, Director of the Astronomical Laboratory, Groningen, Holland.*]

VRIES (NEAR GRONINGEN), *August 31, 1903.*

In answer to the valued invitation of your Committee, I will unreservedly state my views, though they may seem somewhat radical on some points.

For evident reasons there cannot be the slightest doubt that a southern astronomical observatory can do much more for the promotion of astronomy than a northern one.

The works falling in the first line for cultivation at such an observatory I consider to be:

1. Determination of stellar parallax.
2. Fundamental determination of right ascension and declination.
3. Determination of radial velocities of the fixed stars.

There are, most certainly, several other works which urgently call for execution, but I think the three works mentioned must take precedence of all the others. Moreover the chances of these other works being undertaken elsewhere on a more or less sufficient scale seem to be somewhat better. So, for instance, the extension of the astrographic catalogue from declination $-32°$ to $-90°$ in your "confidential statement."

At the Cape the positions of the reference stars for the "Carte du Ciel" have been determined by the meridian circle. This being so, the positions which will be obtained by the measurement of the plates (as soon as they shall have been reduced to right ascension and declination) will make the want of an extension of the astrographic catalogue little felt for that zone.

It seems but reasonable to hope that the existing southern observatories (Cape, Melbourne, Cordoba) will cooperate to furnish the

same material for the reduction of the remaining parts of the southern sky. Therefore, though (with a view to the indefinite time which may still elapse before we get a complete catalogue of right ascension and declination for use from the "Carte du Ciel") I would most cordially rejoice in an extension of the astrographic catalogue, I would still place this work in the second rank of the works most urgently demanded at the present moment.

However this may be, I will restrict my remarks to the three works mentioned, which are the most important of all.

(1) *Determination of Parallax South of Declination —20°.*

As, in my view, there is at present no work so urgently demanded for the advancement of astronomy as the determination of parallax on an extensive scale, the equipment of the observatory for this purpose should be as complete as possible; for instance:

a. Two photographic telescopes, say of 40 cm. aperture and 6 meters focal length. They ought to give round images over a field of 2° diameter.

b. One telescope of 40 cm. aperture and a focal distance as great as is compatible with rigid mounting. A round field of, say, 80′ diameter or even somewhat less will be sufficient.

c. One transit instrument of 7 inches opening.

d. One heliometer of 7 inches.

Regular morning and evening observations demand two observers for each instrument. A small part only of the time of these observers would be taken up by the observations; the rest would be devoted to the measuring of the plates and the reduction of the observations.

To provide such an outfit and such a staff exclusively for the purpose of parallax determination may seem extravagant. I do not think so. The need of a better and more solid knowledge of stellar distance is so great that we should stick to some such plan as is involved in the above, even if it appeared that thereby the funds available would be exhausted. If something must be sacrificed, I think the instruments *c* and *d* could be best dispensed with, as it seems more likely that the observations to be made by these instruments will be taken up elsewhere.

The two telescopes *a* would serve for a photographic Durchmusterung for parallax. Elsewhere (Publications Astr. Lab. Groningen No. 1, pp. 87–98) I have explained at some length the feasibility and desirability of such a plan.

Notwithstanding the reasons adduced (pp. 97 and 98), some astronomers still think that we ought to restrict ourselves to the brighter stars and those of considerable proper motion.

We may now exclude the former from consideration, because, for obvious reasons, a photographic Durchmusterung will do little for the very brightest stars. They must be treated by instruments *c* and *d*, and there will be little difference of opinion as to the desirability of investigating as many of these stars as may be possible.

The real question thus is: Has the time come to make a complete Durchmusterung of parallax for the fainter stars (say 6.0 to 10.0)? Or may we restrict ourselves to stars of sensible proper motion only?

It will be a relatively small undertaking to obtain the parallaxes of the 200 stars of greatest proper motion (this is about the number of stars with proper motion exceeding $0''.6$, known at present, a great part of which are bright ones).

It seems not too much to expect that these will be observed elsewhere. In fact, I think the greater part of them have already been measured. For the rest, if necessary, they will be dealt with by the instruments *b*, *c*, *d*.

Setting aside also the consideration of these stars, therefore, it remains to answer the question: What are we to do afterwards—after the observing of the 200 stars of greatest proper motion?

The more important part of our aim must be to get a knowledge of the distance of a great part at least of our nearest neighbors in the universe, in order that we may begin by making a study of the laws in their distribution and motions. Now, if from the very beginning we exclude all the stars of which the proper motion at right angles to the visual ray is small (and we virtually do this by confining ourselves to stars of great proper motion), then we may foresee at once that the finding out of any real law in the motions will be impossible. Our aim will be defeated from the very outset.

There are other considerations more amply set forth in the paper quoted which must lead to the conclusion that for a study of the construction of the stellar world we cannot escape the necessity of making a Durchmusterung for parallax. It would be a noble task for the observatory to be erected to take the lead in such an undertaking.

For a fairly fine climate I estimate that the two telescopes *a* together would furnish a duplicate set of plates for the whole sky from declination —20° to declination —90° in about eight to ten years.

All the objects found that are suspected to have a fairly large parallax would be taken up by telescope *c*. They would be further investigated only in those cases in which a first plate confirmed the large parallax.

On the plates furnished by this telescope only the principal object, with five or six well chosen comparison stars, would be measured. The work of measuring and reducing would be very moderate, therefore, especially as a reduction with three constants would be sufficient in nearly every case.

Instrument *c* would serve for the parallaxes of the brighter stars (say 0—6) and, together with instrument *d*, for the further investigation of objects of certainly measurable parallax.

The observatory ought further to be fitted out with (say) ten measuring machines. It is very probable that the observers, though their labors at the telescope would take up only a small fraction of their time, will be unable to make the measurement and reductions keep pace with the production of the photographs. It will be necessary, therefore, to procure assistance for them. This, however, will be a question of cheap labor. (See P. S.)

I have dwelt thus long on the subject because it is practically a new one. On the others I have only a few words to say.

(2) *Fundamental Determination of Right Ascension and Declination.*

The great importance of a fundamental determination of star positions, with extension of these observations by secondary methods to include every star brighter than the seventh magnitude south of —20°, is evident to any one who has made some study of stellar motion.

I feel very warmly for the plan of transporting to the southern hemisphere, for a short term of years, one of the reversible meridian instruments of the northern hemisphere. The observations of a considerable number of stars with the same instrument at a northern and at a southern observatory cannot but lead to a material reduction of the influence of systematic instrumental error and error of refraction.

It would seem to me that it would be advantageous to make the determinations of right ascension and declination by two separate instruments, a transit (for which the instrument also used for parallaxes may serve) and a vertical circle. Besides other well known reasons, there is this one: If for the determination of declination we bisect the image of a star by the horizontal wire, its

brightness is very considerably reduced, especially in the case of the somewhat fainter stars. Thereby the personal error depending on brightness must be changed and an element of uncertainty introduced into the right ascensions. If I am not mistaken, the effect was found to be quite perceptible in Leiden.

As the elimination of systematic error influenced by refraction and flexure is of such paramount importance for our fundamental declinations, I would suggest to supplement the above determinations by the determination of some hundreds of fundamental declinations by the method explained in Copernicus III, pages 147–182, which make the result absolutely free from both refraction and flexure. * * *

(3) *Determination of Radial Velocities.*

I have no suggestions to add to the plan developed in the "confidential statement," with which I most cordially agree, as far as I can judge of the matter.

P. S.—In regard to the measurement and reduction of the plates for a Parallax Durchmusterung, which might perhaps be considered to put too heavy a strain on a single observatory, I would like to add that no doubt many of the smaller observatories, not too well provided for, would be only too glad to do the work of first class importance by measuring and reducing good parallax plates. Under certain conditions the laboratory at Groningen would very willingly undertake the complete work of measuring reduction and description for a duplicate set of plates for a zone of 2° width at least.

[*From Dr. Arthur Auwers, Secretary of the Royal Prussian Academy, Berlin.*]

[In a private letter to the chairman Dr. Auwers discusses various topics. The following extracts refer to the plan for a proposed observing station in the southern hemisphere.]

[Translation.]

GREIFSWALD, *October 6, 1903.*

The "confidential statement" enclosed in your letter of July 7 designates the most pressing astronomical tasks to be worked out in the southern hemisphere so fully and, according to my judgment, so much to the point, that I find very little to add thereto; and, furthermore, my pressing work has hindered me until now from pre-

paring an answer. You wish me to express myself especially in regard to Nos. (1), (2), and (4), and, although belated, I will do this.

The *most important* task that today confronts the southern observatories is, in my judgment, the production of *really fundamental* determinations for a selected list of stars. Such determinations for the southern hemisphere are still wholly wanting. We are hoping for a series of such within the next few years from Gill, but it is of the highest importance that we shall be in possession of *several* such determinations, homogeneous and each as a check on the others, and the establishment of a temporary observing station for this purpose would therefore be a timely undertaking.

The employment of a meridian circle, which should be used before and afterwards for similar determinations in the northern hemisphere and which has proved itself to belong to the first rank of first class instruments, would be wholly worthy of commendation. By this means one would at least, to a great extent, remove one element of uncertainty, the adopted flexure. It is possible that the advantage from the employment of this device will not be so great as you apparently hope, since the principal source of uncertainty in our declinations arises from the uncertainty of the refractions, in which local anomalies remain, arising partly in the observatory and partly in its surroundings, and which can be rendered less and less harmful in their effects through increase in the number of observing stations.

The attempts which have hitherto been made to establish an absolute system of declinations through comparison of observations made in opposite hemispheres are founded on the supposition that the refractions on both sides of the zenith are alike, and I doubt whether this supposition is correct for the majority of those observatories upon whose observations we have had to rely up to the present time for the establishment of systems of declination. The correctness of this supposition seems to me especially doubtful in relation to the two southern observatories which, up to the present time, have afforded the most accurate places of the brighter stars. The Cape, as well as Melbourne, observatories have the ocean to the south and a heated continent to the north, and over these different regions there may be very differently arranged masses of air. I consider it, therefore, very important that the new observation station should not have a similar position, but that it should be either purely insular or purely continental. At the same time, the southern-

most latitude possible is desirable. Perhaps the southern part of the Argentine Republic offers what is, on the whole, the most practicable compromise (also the possibility of existence for several years).

As closely related to the fundamental determinations and as a work to be accomplished with the same instrument it is very desirable, as you propose, that there should be complete observation of all stars south of —20° and to the seventh magnitude, inclusive, four times for each star (equally distributed in the two positions of the instrument, and preferably according to the example set by the Pulkova series for 1855, with exchange of objective and ocular). In accomplishing this the period of observation will be scarcely lengthened and a second work of the first importance would be produced.

Of still greater importance than this second work, attached to No. 1, is the continuation of the astrographic zones, the proper and speedy observation of all stars down to the ninth magnitude upon the southern sky (No. 4). In Cordoba this work would have been extended beyond —32°, but since Dr. Thome has now undertaken a part in the photographic chart it is very desirable that others should undertake the continuation of the zones. For this purpose, in addition to the meridian circle for No. 1, a second instrument would naturally be required; but I do not see the use of giving to this instrument a construction of the form you have in mind. The ordinary meridian circle is entirely suited for zone observations, and in any half way favorable climate with such an instrument two observers (one at the ocular and the other at the microscopes) could with ease make 10,000 observations annually at less than —40° of latitude where one has sufficiently long nights at all seasons; two sets of observers could make 20,000 observations annually.

No. 2 is also a task of great importance. The determination of the mean distance of the stars of various orders of magnitude is necessary, in order to provide a firm foundation for investigations into the structure of the stellar universe. But it appears to me impossible to reach this result otherwise than when one investigates the parallax of each single star of the brighter orders of magnitude, together with a sufficient number of the following orders, as far down as the means of measurement will permit. Dr. De Ball has planned such an undertaking and is corresponding in relation to it with others who have heliometers at their disposal—among others with Dr. Elkin. Therefore I will not go into further particulars, and will only remark that it goes without saying that it would be

of the highest value to secure for this enterprise the cooperation of another southern observatory in addition to that of the Cape, this also to be provided with a 7 or 8-inch heliometer. In my opinion, this is the only instrument with which, up to the present time, one has been able to secure reliable determinations of parallax.

* * * * * * *

With these remarks I have desired to express my personal and lively interest in the plans of the Carnegie Institution, for which I wish a speedy and complete fulfilment. I leave to you to communicate to your fellow members of the Committee so much of this letter as you think best, but otherwise I desire that you will consider it a private answer to your communication.

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II. CORRESPONDENCE RELATING TO PROPOSED SOLAR OBSERVATORY.

INTRODUCTION.

In January, 1903, a confidential statement regarding a proposed solar observatory was sent by the Secretary of the Committee to a number of astronomers and physicists. This letter stated that the principal purposes of the observatory, as they then appeared to the Committee, would be to investigate (1) the intensity of the solar radiation and its possible changes during a sun spot period; (2) the problem of the solar constitution, through observations with the spectroscope and other instruments; and (3) various stellar and nebular problems connected with the evolution of the sun and stars. The necessity of choosing sites especially suited for such work was also pointed out, and the suggestion was made that for the study of the solar constant a high mountain station, with a second station near the base of the mountain, might be required. Suggestions were requested regarding the proposed program of work, the selection of sites, and any other subjects connected with the observatory.

In response to this letter the following replies were received:

LETTERS FROM CORRESPONDENTS.

[*From Professor C. A. Young, Director of the Observatory, Princeton, N. J.*]

FEBRUARY 7, 1903.

Naturally I am very much interested in the question of a special astrophysical observatory. There is no question that the lines of

research indicated in your "confidential" paper are important and ought to be followed up in some concerted manner; but I own to some doubt whether it would be best that all that research should be concentrated at any one, two, or even three stations. Cooperation between workers widely enough separated to secure nearly continuous observation might be better, unless some locality can be found where observations are practicable *all the time*, and I know of no such locality, in the United States, at least. Still it is obvious that, given the "ideal" director with adequate means at his disposal, there would be great advantages in the concentration, perhaps quite sufficient to overbalance the disadvantages.

As regards my experience at Sherman, it did not indicate any advantages as to average aerial conditions over Hanover and Princeton, but during my six weeks' stay there (I think it was six weeks) there were two or three magnificent nights, when the conditions were better than I ever saw them here (one night here, perhaps, excepted, or rather a few hours that night). For solar observations, however, the conditions from half an hour after sunrise till 9 or 10 a. m. were fine more than half the time. About 11 it usually began to cloud up, and in the afternoon thunder storms were in order till 8 or 9 p. m., and for some hours afterward the air, though very transparent, was very unsteady. The seeing may have been good after midnight, but I did not examine it often, as my work on the sun gave my eyes all I could safely do with them.

Of course, my statement as to the behavior of the weather can not safely be taken as applicable to all years and months. It was in the months of July and August, 1872, that my observations were made, and I remember that some of the few residents of Sherman said that the conditions were unusual for those two months on account of the unusual amount of snowfall the preceding winter on the mountains west and south of Sherman; but from all I can learn I should think there was much more likelihood of finding better average seeing not very far from the sea, as in southern California.

I ought to add that undoubtedly an outburst of vigorous solar activity on the sun's limb from August 1 to 6 had a great deal to do with my success in finding new chromosphere lines. During the first three weeks of work I made little headway and was much discouraged. When I began to get up and go to work at 5 or 5.30 a. m. things went better, but the days I have mentioned, August 2–5, gave me fully half my harvest.

[*From Professor Henry Crew, Director of the Physical Laboratory of Northwestern University, Evanston, Illinois.*]

FEBRUARY 10, 1903.

I have not earlier replied to your circular letter of the 30th ultimo for the reason that I have nothing of value to contribute in the way of suggestions. Congratulations are certainly due to the astrophysical world on the splendid prospects set before it by the Carnegie Institution.

My own experience, my reason, and my reading all lead me to think that you are not likely to put too much emphasis upon the necessity of untying the bundle of sticks before you attempt to break them. By which I mean to say that you are hardly likely to find at any one station the best conditions for undertaking more than one of the three problems which you outline.

The best conditions for the solution of any of these questions would seem to me something like the following:

1. A carefully and intelligently selected site to which an investigator might go with confidence.

2. A single, definite, and not too general problem; at least, a single problem at a time.

3. The selection of two men whose interest and ability in the matter no one doubts.

4. A simple, plain, but adequate material equipment in all except the central and essential instrument, and then make this the most powerful and most efficient in existence.

5. Study the men you put in charge, see that they are comfortable, but not "too comfortable," and, above all else, see that the conditions (mechanician, etc.) are such that these men's time can all go to the problem in hand. In other words, leave your men "foot free," and then hold them responsible either for results or for difficulties which are certainly insurmountable.

6. Energy without haste; test men and sites deliberately.

Of all the problems which you mention, the most pressing appears to me to be the need of a continuous record of what is going on at the solar surface. Psychologists often find abnormal cases the most instructive. It may be so with solar studies. Next most important appears to me the horizontal telescope of large aperture. I think this deserves a fair trial in the best attainable spot on the globe, both for spectroscopic and for photographic work.

[*From Professor E. F. Nichols, Director of the Physical Laboratory, Dartmouth College, Hanover, N. H.*]

FEBRUARY 12, 1903.

Your letter of January 28, concerning plans and projected work for a new national observatory, has been received. The plans for work embodied under the three heads in the letter seem to me admirable, and to include work of the most valuable kind yet to be done in astrophysics. My own work in astrophysics has been very limited, as you know, but the outlook and far reaching extent of the projected work for the new observatory seems to me in its variety to include nearly everything at present worth doing.

In particular the conditions required for the most successful measurement of the heat radiation of the brighter stars would be a clear and quiet atmosphere for night work, and a large concave mirror of at least 5 feet aperture. The mirror must be so mounted that the beam from the mirror will be reflected in a fixed direction, so that the heat measuring instrument need not be moved in following the star. It is further very desirable, if not absolutely necessary, that the radiometer or other heat measuring instrument may be surrounded by constant temperature conditions during observations. The results which might be expected from such an equipment I have already discussed in my paper in the *Astrophysical Journal*, vol. 13, p. 138.

I shall be only too glad to do anything I can to further the plans of your committee, and only wish that any suggestions that I might make could be based on a broader experience in practical astrophysical work. If at any time I can be of use to your committee in any way, I hope you will have no hesitation in calling upon me.

[*From Sir William Huggins, Tulse Hill Observatory, London.*]

FEBRUARY 17, 1903.

I am very glad to hear that there is some prospect of establishing new observatories to provide for observations and researches which require special conditions of position or of equipment.

The lines of work sketched out in your letter appear to me to be admirably thought out, and indeed so complete from the point of view of the investigations which have the more immediate claims that I do not see that there remains much, or indeed anything, for me to suggest.

I think that it is of first importance to have a permanent observatory furnished with a large *reflecting* telescope and a complete equipment of auxiliary instruments for astrophysical research on some site with the most favorable conditions of atmosphere. If this were near the equator, it would command the richest regions of both hemispheres. I mention this point in case it might not be found possible to build a separate southern observatory.

The observatory next in importance, it seems to me, would be one on the top of Mount Whitney, devoted especially to solar work, or chiefly so. I think the photographic method of getting the corona should be tried. Theoretically it is certain of success, if only the atmospheric conditions are but a little better than normal surface ones.

I suppose work could only be carried on during the summer, but if the conditions are as good as the altitude would suggest, there is certainly work enough for many years to come.

The observatory at the base might be regarded as temporary, and perhaps might be given up when a sufficient number of observations simultaneous with similar observations at the observatory on the top had been made.

[*From Professor Arthur Schuster, Director of the Physical Laboratory, Owens College, Manchester, England.*]

FEBRUARY 18, 1903,

In answer to your letter of the 28th January: I should, of course, be highly pleased if funds were to become available for the important work sketched out by you. Taking the different points of your letter in order:

1. I have recently looked carefully over a good deal of literature concerning solar radiation, and I confess I have not been impressed by the probability that simultaneous observations at high and low altitudes will help us very much. The differences which such observations could show would all be due to the layer of air included between the two levels. On different days the atmospheric conditions of that layer may be very different, and yet the atmospheric conditions at greater heights than, say 15,000 or 20,000 feet, might be the same. You would at once have errors introduced, and the observations at the high altitudes might by themselves give you better results than the combination. You must face the fact that it is impossible altogether to eliminate changes in atmospheric condi-

tions. Supposing that you were to come to the conclusion that the solar constant observed, even at high altitudes, varies with a sun spot period, it would still be open to doubt whether the change is not due to something that happens in the higher layers of the atmosphere. That, in my opinion, would be the most probable explanation, but as even this fact would be important to establish, I quite agree that observations on the solar constant should be made at frequent intervals.

Whether there are any actual differences in solar radiations at different times is a question that will, I think, be solved in a different manner. It seems to me exceedingly unlikely that any increase or diminution in solar radiation can take place equally and simultaneously all over the solar surface. If sun spots have anything to do with it, we must imagine the changes to come out differently or at least at different times in different solar latitudes. I would therefore consider it a matter of first importance to improve observational methods as much as possible, so as to be able to compare with the utmost accuracy different parts of the solar surface in different portions of the solar spectrum. A change in temperature of even 100° ought to make an appreciable difference in the radiation at the violet end, though the radiation in the red will not be affected nearly as much. I have been very much struck with a recent paper by Mr. Wood, of Baltimore, which describes a screen that absorbs the visible light, leaving the ultra-violet. I should say that simultaneous photographs taken of the solar disc by the ordinary method and with this screen might give interesting results.

2. I think you know already the great importance I attach to the careful investigation of the spectrum of sun spots, and the other points you mention are of equal interest.

3. Here again I agree with you that such work as the measurement of the heat radiation of stars and investigations of spectra with very high dispersion are bound to lead to important results, and if your present atmospheric conditions at available observatories are not sufficiently good it would no doubt be highly advisable to have a site specially selected for the purpose.

I am not, of course, able to judge whether the same site may be suitable for the solar and stellar work, and I do not want to discourage altogether the possibility of a station at the base of a mountain on which solar observations are taken. Some useful results might be obtained in this way, but I should not hope for very much and should not personally be inclined to recommend any great ex-

penditure for such a purpose. Possibly a very moderate equipment at the base would do all that is desirable.

Might I also mention, in addition to the matters you speak about, that a repetition of Dunér's work on solar rotation in the reversing layer is, I think, called for? I do not think the last word has been said in that matter.

[*From Professor H. C. Vogel, Director of the Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

FEBRUARY 19, 1903.

In reply to your kind letter of January 15 in regard to the establishment of a great astrophysical observatory with stations at particularly favorable points, I wish to reply that I can only commend in the warmest way the realization of this great undertaking in the interests of science, and that I regard the investigations proposed by you, on the subjects named below, as so suitable and exhaustive that I am unable at present to suggest anything further:

(1) Solar radiation problems, involving the measurement of the solar constant at frequent intervals throughout the sun spot period.

(2) Solar investigations, principally of a spectroscopic nature, which require atmospheric conditions and instrumental facilities superior to those hitherto employed.

(3) Various stellar and nebular problems, such as could be undertaken to the best advantage with the aid of a large reflecting telescope.

In Europe we are not so fortunately situated as to be able to find so easily sites from which the proposed observations could be carried out to advantage. We can therefore only wish success to our American colleagues if the Carnegie Institution should provide the means necessary to carry out such comprehensive investigations, which are so important for astrophysics.

[*From Professor H. Kayser, Director of the Physical Institute, University of Bonn, Germany.*]

[Translation.]

FEBRUARY 20, 1903.

I am so busy at the end of our semester that I can reply only briefly to your letter of January 30.

If a new institution, provided with unusual instrumental means, is to be established for the study of the sun, it should have the solution of one especially important and fundamental problem as its principal purpose. The most important question seems to me to be with regard to the constitution of the sun. We seem to be infinitely far from the solution of this question—whether it is wholly gaseous or in part liquid or solid. This question must be solved through a very detailed study of the various parts of the sun. It seems to me particularly important to make a study of the sun spots. If their spectra could be photographed in the greatest possible detail throughout a solar cycle with large gratings, an important advance would probably be made. In this way it might be possible to form a proper estimate of Julius's theory—either to confirm or refute it. This investigation is a rather thankless task, since it consists only in gathering observational material; but a result drawn from it would be far more important for our knowledge of the heavenly bodies than the measurement of the radial velocity of a few hundred stars or the intensity curves of variables. As soon as the characteristic features of our sun are thoroughly understood, many other phenomena will explain themselves.

I would advise the use of plain gratings for this investigation, since freedom from astigmatism is necessary; the lenses should be preferably of quartz-fluorspar, and the solar image on the slit should be of large diameter.

Terrestrial spectra must, of course, be employed for the explanation of the phenomena. The laboratory must, therefore, be supplied with concave gratings of various numbers of lines to the inch and various radii of curvature; also with direct current for arc lamps and alternating current for transformers, as well as induction coils of various dimensions up to one meter spark length, in order that the spectra of the elements may be studied under the most varied conditions.

I have touched upon only a single question, which seems to me to surpass all others in importance. It goes without saying that a large institution would also undertake investigations requiring night observations. I name here briefly only two of investigations which seem to me particularly important: the spectra of the planets, in order to determine the nature of their atmospheres, and the spectra of a number of the brightest stars with a precision approaching as closely as possible that of Rowland's solar spectrum, so that their chemical constitution can be accurately compared, and if differences

are found in the spectrum of the same element the cause of these differences may be investigated.

I believe that the problems which I have named have hardly received serious consideration hitherto. All are for the purpose of determining accurately the physical and chemical constitution of at least a few of the heavenly bodies. In the case of the sun itself Rowland's atlas and tables may be regarded as only the first though a most important step toward a knowledge of its chemical constitution. A further study of the spectra of the elements would certainly permit 90 per cent of the lines now designated as unknown to be identified.

[*From Dr. W. E. Wilson, Private Observatory, Daramona, Ireland.*]

FEBRUARY 24, 1903.

I was very glad to receive your letter of January 28 and to see that there is the prospect of founding a large observatory for investigations on solar radiation and kindred subjects. I am sure such research can be profitably carried on only in situations such as you propose. Ireland is certainly *not* one for such work. I have been experimenting for two years with a new recording bolometer, which I think would give excellent results if it were mounted in a situation where it would get some sunshine. This it does not get here. If you would care to have it tried at your new observatory I would be delighted to send it to you. All you would require would be one of Callendar's electric recorders to work with it. It consists of two flat coils of platinum wire blacked and sealed up in an exhausted glass tube. This is enclosed in a brass tube with suitable diaphragm, so that one coil only receives radiation from the sun and a small bit of surrounding sky. The other coil is in the shade. These form the arms of the Wheatstone bridge of the recorder, and it gives a continuous record of the radiation. With the form of receiver designed by Callendar the coils were not in vacuum, but merely covered with a glass shade. One coil was black and the other bright, and both lay horizontally, so that the sun was never normal to them, and of course changing from hour to hour. I found that with this old receiver it was quite impossible to calibrate the curve with Ångström's pyrheliometer. Even on a dark, wet day it gave a considerable deflection, by reason of the glare from the clouds. With my new form the curve can be calibrated with the Ångström instrument perfectly, and by means of a planimeter, which is attached to the pen of the re-

corder, you can read every day the area of the resulting curve. I enclose you some sample records. Those marked A are taken with the old receiver and B with the new one. You will see that in B when there is no sunshine the curve falls to the zero line. In A it is always above it.

I wish you could also see your way to carry on a series of observations by allowing an image of the sun to transit over the aperture of a small radiomicrometer and recording the deflection on a moving photographic plate. By a discussion of the resulting curves I think it would be quite possible to determine whether the depth of the absorbing layer on the sun varies during a sun spot cycle. I began taking curves here, but the weather was so hopelessly unfavorable that I gave it up.

[*From Sir Norman Lockyer, Solar Physics Observatory, South Kensington, London.*]

FEBRUARY 25, 1903.

In response to your letter of January 29 regarding the work to be done by a new astrophysical observatory, I would say that your scheme practically covers the whole ground. There are, however, one or two points to which attention might be drawn:

1. Would it be advisable to erect a permanent building on a mountain summit without first putting up a temporary building and making observations from it for a year or two?

2. It is of great importance that the observatory should not be too far away from some large town easy of access, as modern work requires the investigator to be in touch and personal contact with scientific men for purposes of mutual assistance and advice in addition to the reasons given in your letter.

3. It is important that the laboratory equipment should be complete, for it is the mutual work of the observatory and the laboratory which helps the investigation.

4. The Janssen-Hale-Deslandres method of photographing the solar prominences should undoubtedly be undertaken and made strict routine work.

I am very glad to see that you mention the photographing of the ultra-violet end of stellar spectra, as this is important and we are at work upon it, although we have very small means here for carrying it on.

[*From Professor C. E. Mendenhall, Physical Laboratory, University of Wisconsin.*]

FEBRUARY 27, 1903.

Your letter of February 2 came duly to hand, and I have made a few notes concerning that part of the proposed work with which I am somewhat familiar. These I am very glad to send you now, hoping that the delay has been of no inconvenience to you. I have not thought that you wanted great detail, and have tried to avoid it. Though most if not all of the suggestions are such as must have already occurred to you, I nevertheless give them, thinking that perhaps they may usefully serve to confirm if not to initiate.

As regards the general scope of the work, I shall not presume to speak further than to emphasize the importance of one point which you mention, namely, provision for the study of such laboratory problems as seem intimately connected with the solar and stellar work.

In deciding upon the site it may be well to keep in mind, besides the primary requisites, the fact that the observatory will demand a constant though small supply of power. Possibly in California some long-distance transmission line or water power could be drawn upon.

For the solar constant work examples of all the best forms of pyrheliometers and actinometers should be provided and studied with a view to improvement. No one of them seems independently reliable at present, though the Ångström compensation pyrheliometer promises best.

For detailed infra-red spectroscopic work there is no doubt that the bolometer is the most immediately applicable, because of the work of Abbot and others at the Smithsonian Astrophysical Observatory. But the radiometer, thermopile, and radiomicrometer each has its peculiar advantages, so that if they were properly modified for linear spectroscopic work they might be used with advantage at the two substations. It seems to me unlikely that they will ever supplant the bolometer for accurate linear work in cases where the best must be had at any cost.

In designing the spectrobolographic outfit it seems to me questionable whether it should be as large as that of the Smithsonian Institution. At any rate, even if one outfit of such size is provided for the main station, it would be desirable to provide another of considerably smaller size, more or less self contained and capable of being used with advantage by a single observer. For some of the work

this could be used in place of the larger outfit with resulting saving in time and labor, and for much of the preliminary work and for the laboratory investigations it would be decidedly more convenient.

A storage battery equipment will be a necessity, small and portable, for the substations, but of considerable capacity for the main observatory. Especially should this be the case if laboratory work is undertaken, as this will undoubtedly involve the production of high temperatures which can best be done electrically. In this case also accurate means of measuring high temperatures will be necessary and a variety of laboratory sources of radiation should be available.

Of course, work would be greatly facilitated by a generous assortment of miscellaneous laboratory apparatus, such as small spectroscopes, telescopes, mirrors, polarizing apparatus, air-pumps, etc., and a lot of laboratory supports and attachments. Finally, as much of a shop equipment as is possible.

In connection with the spectrobolographic work, I must confess that at present the further detailed study of the infra-red solar lines does not seem to promise very much. Undoubtedly the true solar lines can be separated from those of terrestrial origin and identified to a greater or less extent with emissions of known elements, and this would perhaps be of most value in connection with the question of the persistence of given emissions through long ranges of temperature. Again, if the sun's surface were studied in greater detail the infra-red lines might help in the study of motions in the solar atmosphere, but it does not seem likely that they would be more important for that purpose than the lines photographically observable. Of course, it *may* be found that the infra-red lines behave in some ways quite differently from those of shorter wave length, and hence furnish a valuable tool for solar investigation, but it does not seem to me that the work so far done leads one to expect this. However, you know much better than I what to expect. Again, is it not true that the solar constant work and the separation of true solar lines from those of terrestrial origin are the parts of the work which really demand the high and low mountain stations, while much, if not all, of the other work, on account of its more intimate connection with laboratory experimentation, could better be carried on at a more centrally located observatory; for example, the Yerkes, best of any.

[*From Professor A. Riccò, Director of the Royal Observatories of Catania and Etna, Sicily.*]

[Translation.]

FEBRUARY 28, 1903.

With regard to the project under consideration by the Committee of the Carnegie Institution, of which you are a member, it is certain that if a station at the summit of a very high mountain is needed for the study of the solar radiation, another station will also be required which may be conducted without encountering the difficulties which are unavoidable at elevated stations; among others, the frequency of days when the mountain is enveloped with clouds due to the condensation which it produces. Etna, for example, as seen from Catania, is enveloped with clouds 167 days in the year, on an average.

The other station should not be placed at the foot of the mountain, since it would also experience the effect of condensations caused by the mountain. At Catania the clouds cover on an average only 39 per cent of the sky (Palermo, 46 per cent), but this amount of cloudiness would be still smaller if it were not for Etna, since the clouds appear more often on the side toward Etna (north) than elsewhere. Furthermore, the station at the base of the mountain would not have an entirely free horizon in one direction.

There should also be a third station on a very extensive high plateau on a small island, in order to have a very homogeneous atmosphere in perfect equilibrium, and to avoid disturbances of the images caused by ascending currents; these are very pronounced during the hotter part of the day on Etna, and carry up with them visible vapors, which frequently hide the sun, and invisible vapors which produce remarkable absorption of the solar radiations. On Etna the curves of heat received by the Arago actinometer have this form [figure not reproduced here]. This may be explained by the absorbing action of the ascending vapors during the hours of greatest heat. At Catania the curves have the regular form, rising with the altitude of the sun.

At Catania, as at Palermo, the images of the sun are best in the early morning; ordinarily during the hot seasons they are bad at 10 o'clock; they become better again before sunset.

[*From Professor A. Belopolsky, Imperial Observatory, Pulkova, Russia.*]

[Translation.]

MARCH 11, 1903.

The project of constructing several astrophysical observatories in the United States is of very great importance. Investigations of the sun are precisely those which require study by modern methods. At the present time, since the discovery which you have made with the spectroheliograph, the study of the solar surface with instruments of sufficient size promises to reconstruct current ideas regarding the constitution of the sun. The glory of accomplishing this will belong to the United States if the construction of special observatories is provided for.

It will perhaps be possible to find a mountain more advantageous than Mount Whitney for investigations of the solar radiation. I believe that the station can be established only at a distance of 300 meters below the summit. The plan for the investigations on the constitution and radial motion of stars also requires instruments more powerful than those which are employed at the present time. Everyone desires to undertake such work, but no country can realize this desire, since nowhere are sufficient funds available for scientific researches. It is only in the United States that private fortunes are devoted to science.

But, as you are well aware, successful investigators are quite as necessary as instruments. I believe that as many scientific investigators as the newly established observatories will need may also be found in America.

[*From Professor Cleveland Abbe, U. S. Weather Bureau, Washington.*]

APRIL 3, 1903.

Your letter of March 25, as Secretary of the Commission relative to the establishment of a large astrophysical observatory, interests me very much. The practical question as to the location and maintenance demands first consideration. There is no doubt that an observatory at the highest practicable point, working in cooperation with one lower down, will eventually add much to our knowledge of the solar and the terrestrial atmospheres. I consider Mount Whitney the most desirable summit station; a station at its base is necessary both for supplies and for special work on absorption. Another station in nearly the same meridian but farther south can

scarcely be found, but in place of that I think that the location of the Flagstaff observatory would be an admirable substitute. Among the solar investigations, I hope an effort will be made to get at the differences in radiation from different spots on the sun's surface in their successive rotations, so as to get the chronological variations in temperature, as well as the geographical differences. The complete course of work that you have sketched out covers all the problems that have thus far been found worthy of study, and, of course, the observatory will take up new ones as fast as they develop.

I should not encourage duplicating in the southern hemisphere all the investigations that are to be conducted in the northern, but there are some problems that could advantageously be studied at both observatories. On account of our knowledge of atmospheric conditions at the stations Charcani and El Misti, that location has some advantages, but there is still hope of finding an equally advantageous location on the mountains of Ecuador or southern Colombia. When such a station is found, it should be devoted especially to studies on nebulæ and stars, such as are described in your article 3, and perhaps also to less extensive studies on the determination of the solar radiation constant.

From a meteorological point of view, observations on these northern and southern mountain stations are extremely desirable, and especially if observers at the upper and lower stations can make absolute determinations of the altitudes and motions of the clouds or temperatures of the upper air by means of kite and balloon ascensions. Many other studies into the physics of the atmosphere, such as its gaseous constituents, its dust, and its motions, would constitute valuable additions to our meteorological knowledge. The special field to be occupied by such an observatory relates to the highest attainable atmospheric strata.

If there is any specific matter on which I can be of use to your Committee, I shall always be happy to respond.

[*From Professor G. Müller, Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

APRIL 4, 1903.

The plan to establish a great astrophysical observatory at a particularly favorable site, and to provide it with the best instrumental equipment, will be greeted everywhere with lively interest. For

many problems of astrophysics the atmospheric conditions experienced at most of our observatories are to be regarded as unfavorable. Every increase in the transparency and also in the steadiness of the air marks an advance, and for this reason the choice of a site for a new astrophysical observatory should be made with the greatest possible care.

The best conditions of the atmosphere are to be expected at a mountain station, and, according to my experience, an isolated peak is to be preferred to a point within a great mountainous region, where surrounding peaks under certain circumstances exercise a strong influence on the state of the atmosphere.

The higher the chosen point the better, but height is not the only important factor. It is far more important that the observatory shall not be too difficult of access, and, before all else, it is essentia that the observations can be made with as great convenience andl ease as at any other observatory. Occasional observations, such as are made during a very short period on a very high mountain under the most difficult conditions, are ordinarily of comparatively little value. Many problems of astrophysics, such as the determination of solar radiation and investigations on the absorption of the atmosphere, etc., cannot be solved during short expeditions, lasting days or weeks; they demand systematic study during a long period of time under the most varied atmospheric conditions—if possible, simultaneously from a peak and from a valley station.

In my opinion, it would be best to establish the proposed principal observatory as high as possible, perhaps at an altitude of about 8,000 feet, but in any event so that it could be kept in operation throughout the entire year, or at least through the greater part of it, and be at all times accessible without too great difficulty. At this place the principal instruments should be established, and all investigations conducted which relate to the spectroscopy and the photometry of the fixed stars, particularly those in which photography is employed. An important requirement is the provision of a second *permanent* station at a height of about 1,000 feet. This should likewise be provided with the best instrumental equipment, with the object in view to provide for certain observations which should be made simultaneously at both stations. It is naturally desirable that the direct distance between the two stations should be as small as possible, and that they should be connected with each other by telegraph and telephone.

As regards the program of investigations prepared by your Com-

mittee, I think that this covers all fields of astrophysics in an exhaustive way, and that hardly anything of importance can be added to it. I beg briefly to call attention to a few special investigations which, in my opinion, deserve special consideration:

(1) Investigation of the atmospheric lines of the solar spectrum by simultaneous observations at both stations under the most diverse atmospheric conditions and at all times of the year.

(2) Determination of the extinction of starlight by simultaneous photometric observations at both stations.

(3) Thorough investigation of the photographic extinction by simultaneous photographs of given groups of stars.

(4) Determination of the absorption for various regions of the solar spectrum by spectro-photometric measures.

(5) Comparison of the light of the sun with that of the moon, the planets, and the fixed stars, to determine by continued observations whether any variation in the intensity of sunlight can be detected.

In conclusion, I heartily wish you success in the great undertaking, and trust that your plans will soon develop in the interests of our science.

[*From Professor J. Hartmann, Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

APRIL 12, 1903.

Let me extend to you my heartiest congratulations on the astonishing advances which astrophysics has already made in America. It must be acknowledged without envy that the new continent has wholly outstripped the old one, and I would regard the establishment of a high altitude observatory as a glory to American science. When, as here in Potsdam, one is forced to observe with a great refractor under very bad atmospheric conditions, one soon comes to appreciate the enormous advantages of a high station with transparent air and quiet images. I will mention here one point in particular regarding which I have had some experience. If a telescope is to be used for spectrographic investigations it is necessary to have the greatest possible aperture in order to secure great light grasping power. If the ordinary ratio, about 1:18, of aperture to focal length is employed, this large aperture corresponds to a very great focal length, and in consequence of this the images become so bad with unsteady air that the advantage of the great aperture is almost

wholly lost. In this way, *in an unsteady atmosphere*, our photographic refractor of 32 centimeters aperture gives practically the same results as the great refractor of 80 centimeters aperture. If a greater ratio of aperture to focal length is chosen, say 1 : 10—quite apart from the great thickness of glass required in the refractor—new difficulties result, in that the aperture of the collimator can be only 1 : 10, and consequently for a given size of prism the collimator will be too short.

At a high station these several difficulties are not to be feared. If you find a station with very quiet images, I should recommend for spectrographic work—either with very high dispersion on the brightest stars, *e. g.*, for the determination of the solar parallax, or with smaller dispersion on faint nebulæ and stars—a reflector with great aperture, and a ratio of aperture to focal length of about 1 : 30 or still less. I should also choose such a construction as to permit the spectrograph to remain in a constant position, with the collimator horizontal or in the direction of the earth's axis. Such a spectrograph with long collimator, very short camera, and very high dispersion would be best adapted to carry out the very important investigation on the motions within nebulæ to which I recently called attention. Our apparatus here did not permit me to accomplish much in this direction. If the spectrograph were built in a fixed position, not suspended from the eye end of the telescope tube, it could be made much more stable and also easily maintained at a constant temperature. It would thus be possible to employ very long exposure times. A horizontal mounting is also to be recommended for spectroheliographs of the largest dimensions. It is important to make these photographs with a very large solar image, in order, for example, to be able to study with precision the motions within a sun spot. Spectrographic studies of the zodiacal light and the aurora should also be included in the program.

I heartily wish success to the great undertaking, and it will give me pleasure if I can aid it in any possible way.

[*From E. Walter Maunder, Esq., Royal Observatory, Greenwich.*]

MAY 14, 1903.

With regard to the question of astrophysical research, my own position has led my thoughts in two directions.

To me sun spots seem to be the most important subjects of study. Our work at Greenwich consists, as you well know, in taking two

photographs of the sun daily on a scale of 1 decimeter to the solar radius and measuring one of these for the area and position of the spots. So far I think we fulfill our purpose sufficiently well. A larger scale is not necessary for positions of the accuracy we seek; indeed, the scale of 1 decimeter to the solar diameter was sufficient for that purpose and was less costly and the photographs were more manageable in size. To attempt to push the work of measurement and reduction to a further refinement would immensely increase the cost, and I doubt whether it would repay the outlay and trouble.

But when we come to the question of the details of the spot forms and of their changes, then this scale is certainly not adequate. I greatly wish, and have done so for years, that we had here a second telescope with which we could take comparatively small areas of the solar surface on a scale of at least 1 meter to the solar radius. I think this is needed to supplement the other.

But the study of spot spectra is much more urgently needed. My own slight experience of it with a most hopelessly inadequate instrument was sufficient to make me feel that it was absolutely one of the most important lines of research.

I have always felt it a great misfortune that Sir Norman Lockyer, in the work which he has carried on at South Kensington for so many years, should have devised the method of recording just the "twelve most widened lines." It seems to me in every way a badly devised scheme. If a long series of observations are conducted by one and the same observer, I should think it ran a great chance of stereotyping more or less accidental impressions. If the observer is often changed, we have no longer any means for comparing observations made at different times; and, at best, if we assume the observations free from all personality and absolutely immaculate in quality, they seem to me to tell us hardly anything at all. The general character of any particular spectrum—not to speak of important details—is left absolutely without record in such a system.

Sir Norman Lockyer's chief result, namely, that the most widened lines change with the progress of the spot cycle, opens out a great number of questions. First, the spots at maximum are not only more numerous, but they run much larger than at minimum. It would be most important to observe both at minimum and at maximum a series of spots of a definite size. I would suggest, as it is a size sufficiently frequent even at minimum, a spot of 200 to 300 millionths in area. Clearly it is a great assumption, if we find a

certain spectrum given us by a spot of area 200 at minimum and another spectrum given us by one of 2,000 at maximum, to ascribe the change to a quality in the period (if I may so express it) when it may be a function of the size of the spot itself. Further, the average spot group goes through a certain pretty well defined routine in the course of its growth and decay. Now, if we are at the minimum of the cycle, our groups run small, and it is only (in most instances) during one particular phase of its development that a group is likely to be a tempting object for spectroscopic examination. At maximum we may have plenty of giant groups, which can easily be followed spectroscopically during their whole career. Here, again, is a point which wants to be followed out. If we record a given spectrum for a certain spot, we have not learnt all that we can unless we trace the history of that spot back to its rise and onward to its disappearance, and determine at what particular stage of its development the observation was made.

We want to know whether we can associate different spectroscopic appearances with—

(1) The size of the spot; since the larger spots may be assumed on the average to be the deeper.

(2) The stage of its development. The depth may alter with the age.

(3) The changes that are going on in the group.

(4) The progress of the general solar cycle.

(5) The type of the spot group.

I have put the type last, out of its logical order, because it has seemed to me that, though the great majority of spot groups conform to one general type of evolution, yet occasionally we get spots of a very distinguishable form, and it is these spots, when of immense size, and not spots of the normal type, that are clearly and unmistakably associated with magnetic storms. It would be no small matter if we found that such spots exhibited some distinct spectroscopic peculiarity.

As to the method of observation, clearly the photographic registration of spot spectra should be the routine one; but it certainly should not exclude the direct visual work. Just precisely as our daily photographs of the sun's surface at Greenwich, however admirable for their purpose, leave us without any record of the processes of rapid change, so it would be with the photographic registration of sun spot spectra. They cannot possibly render direct work unnecessary.

Might I quote from a letter to me from Mr. Evershed on this subject. He writes:

"We are apt to become too familiar with sun spots to be surprised at their occurrence; but I am sure that when their origin and meaning is really understood, a key will be found to a great many other solar and stellar problems. I consider that as a preliminary the spot spectrum should be investigated with a bolometer in the visible region and infra-red to find out whether the discrepancy between thermal and visual estimates of spot darkness is real, and, if so, where in the spectrum is the excess of radiation measured thermally.

"Another point to clear up is the resolution of parts of the spot band into lines observed by Young and Dunér. Does this apply to all spots and to all parts of the spectrum, and is the emission spectrum of the photosphere itself really continuous under high dispersion?

"It seems to me that until the fundamental radiation of spots is satisfactorily cleared up the study of widened lines is of secondary importance."

The other subject to which my attention has been directed is the study of Jupiter. It seems to me that that planet ought not to be left to the scrutiny of amateurs, but should be systematically observed at some permanent and endowed observatory. As the sun is the only hot star which we can study in detail, Jupiter is the only cold star, and we are fortunate in having representatives of both ends of the series within our reach. In my connection with the British Astronomical Association, my attention has been drawn to the special field for work which this planet offers. The Association has done what it could in the matter. Its object was the training of amateur observers, and their direction to real systematic purposeful work in place of the desultory star gazing which is too often all that amateurs achieve. So far it has been most successful, and the Jupiter Section numbers quite half a dozen observers of the very first rank, beside others who may in course of time attain the same skill. But an association such as ours can never be a substitute for a permanent observatory. A very large proportion of its efforts must be spent in the work of training; there is no guarantee that any of its observers will be able to follow up a research for a long-continued period, and the means for the proper discussion of observations are quite lacking. I might mention, as an example, one of our most active students of Jupiter, Capt. P. B. Molesworth, R. E., of Ceylon. He has been working there for nearly eight years, and in a single apparition has obtained nearly 4,000 transits of spots

across the central meridian of Jupiter, revealing not a few interesting relations; but necessarily he will not be able to remain much longer at that station, nor can he give to his observations the full discussion they deserve. If work similar to his could be undertaken by a professional astronomer, who would have the time to fully discuss his results, at a permanent observatory, which would secure continuity to the work, I think it would ere long lead to our understanding the condition of Jupiter far better than we can at present. Obviously an equatorial or at least a tropical site for such an observatory would, as Captain Molesworth has found, have great advantages.

[*From Professor Knut Ångström, Royal University, Upsala, Sweden.*]

MAY 16, 1903.

I beg you to excuse my long delay in answering your very interesting communication on the astrophysical observatory planned by the Carnegie Institution, but a great many duties have hitherto made it impossible for me to reply. I hope, however, that you will not see in that delay a proof of indifference regarding a question that in fact interests me profoundly.

Unfortunately I can give no information as to the site of the observatory, having no personal experience with regard to the atmospheric conditions in the mountains of California. The establishment of two corresponding observatories at different heights will certainly be most valuable for the scientific results. As to the choice of a place, it will probably be of great importance to study the local conditions. My experience, derived from visits to Teneriffe, is that on the northern side of the mountain it is almost impossible to get satisfactory results in solar observations, while the southern side is very favorable for that purpose.

As to the instruments for measuring the total radiation of the sun, I am sure that the compensation pyrheliometer is at present the only convenient instrument, and I am willing to superintend the construction of the instruments that the observatory may think proper to order from the mechanician Rose in Upsala.

Probably the program of the observatory comprises also the registration of spectral energy by means of the instrument of Professor Langley. I may, however, call your attention to the advantage of making the registration also with another instrument, with less dispersion, which in a shorter time could give a general view of the

solar spectrum and of its changes. I send you an account of a simple arrangement for that purpose.

It would be of special interest if these observations could be combined with researches on the amount of humidity in the free atmosphere (by means of kites). I hope to return later to certain questions that I believe to be of great importance. It will always be a great pleasure to me if I can be of service to you.

[*From H. F. Newall, Esq., The Observatory, Cambridge, England.*]

CAMBRIDGE, *May 22, 1903.*

I almost hesitate to put down some of the ideas that have occurred to me about the establishment of a large astrophysical observatory, for they are rather crude, incomplete beginnings than formed judgments, and if I put them down at all it is only in the hope that they may help to clear matters rather than with the idea that they can carry any weight.

First of all, let me say it seems a grand project to provide for an observatory for observations of *secular* physical phenomena of solar origin. It is perhaps an open question whether it is desirable to provide for allied stellar studies. Many people must share the same instinctive doubt about a universal observatory as about a universal instrument. One may definitely count on individual enterprise to provide for many of the researches indicated in your paragraph (3). Existing observatories do or can or should deal with most of the studies referred to, and it would be a pity in any way to risk cramping either performance or obligation in these matters. Moreover, there are the elements of competition; as, for instance, in determination of motion in the line of sight for *fainter* stars one may count on advance from existing observatories.

If in your large astrophysical observatory large special apparatus were available it might well be desirable to let the place become, as it were, a court of final appeal, whither perhaps rival pioneers might themselves resort to put their views to the test. In such cases, for instance, it might be a matter of "more light" being needed, and the pioneers might be expected to take with them their own eye end apparatus for attachment to a large light grasping instrument; but one would like to provide in every way against retarding small private enterprise and pioneering elsewhere. One must avoid anything that would lead to the position, "Oh, it is no use doing this or that; they have that on their program up there."

Where natural competition is absent or periods of phenomena are long, there is a grand opening for a powerful combined attack. One may count, for instance, on special studies of solar rotation at existing observatories, simply because the period is short, but probably the variation of such short period phenomena in the eleven year period could only be properly tackled in an observatory where the outsetting aim was the study of secular phenomena.

Hence it seems to me that secular phenomena are the special province of such an observatory as you are contemplating; and it would seem a wiser course to concentrate attention on such observations as would have direct bearing on these, and to provide for a systematic discussion of observations already accumulated, as well as for a systematic study of phenomena in process of being observed, than to scatter forces on the study of many stellar problems.

In many ways probably more advance could be made by enabling a single observer to carry out his observations in several stations successively. The solar radiation "constant" is an instance in point. Considerations of this sort would lead me to think that in some ways it might be better policy not to lock up huge capital in one fine observatory outfit, but rather to help individual researches by providing means for having them carried on with, say, one or two complete outfits that could be moved to various points of the globe.

As I say, I hesitate to commit these remarks to paper. I suspect you are far beyond the elementary stage that these remarks refer to.

As to aims and researches, your program is a large one already. It is not clear to me why it should not include a new attack on magnetic disturbances, and possibly on atmospheric and electrical phenomena.

[*From Dr. Ralph Copeland, Astronomer Royal, Royal Observatory, Edinburgh.*]

AUGUST 15, 1903.

I am afraid you will think me remiss in only now replying to your letter of March 26; but I have indeed most carefully thought over your project and looked up my old papers on mountain observatories. I have not much to add to the views which I expressed in my paper on the subject in volume III of *Copernicus*, which you have; but, when consulting it, kindly substitute on page 230, line 22, 1.32 inch for 0.7 inch.

Another note on my South American trip, written for the British

Association Report for 1883, may interest you, and I therefore inclose a couple of copies.

My own impression is that to reap the full benefit of a mountain station one should aim at a height of fully 11,000 feet, and if well within the tropics an elevation of 12,500 feet might be occupied throughout the year withont serious discomfort. Such an altitude in either temperate zone, however, would expose the observers to the most terrible weather and great hardships in the winter—*e. g.*, the floor of the Crater of Elevation of Teneriffe (7,200 feet, latitude 28¼°), according to the late Professor C. Piazzi Smyth, is swept by violent snowstorms every winter. The experience of residents and travelers in your own mountains will furnish you with abundant further examples.

My experience at Puno on Lake Titicaca (12,500 feet), in latitude 15° 50′ south, proves that observations can there be carried on under favorable conditions of weather and temperature at all seasons of the year. Indeed, the sky, on the whole, is much clearer in the winter, and therefore better suited for observations in general, although there are doubtless certain solar investigations which could in that latitude be better prosecuted at a season when the sun passes within a few degrees of the zenith for many weeks in succession. In the months of October, November, and December the weather is often very fine, I was told; but in January, February, and the early part of March clouds, and even a good deal of rain, are to be expected.

As you will know from *Copernicus*, my experience in the Andes was confined to the neighborhood of the Mollendo-Puno route, where, through the courtesy of the railway authorities, mechanical and technical aid is readily procurable; but doubtless the same mechanical facilities would be offered on the Oroya railway, which, starting from Lima, in 12° south latitude, reaches a height of fully 15,600 feet quite near the Pacific seaboard. Unfortunately the disturbed political state of the country at the time of my visit prevented me from examining this railway, but from its position so near the rainless coast it is very possible that the weather conditions near the upper part of the route may be fully more favorable than on the Mollendo railway.

But I should here like to draw your attention to a point affecting the personal comfort and even safety of the members of an astronomical party on their way to a high-level station. In my opinion, the whole of the ascent should not be attempted on one day; the party ought to devote something like a week to inuring themselves

to an elevation of 7,000 to 10,000 feet before proceeding to the more trying height of 14,000 or 15,000 feet. In my own case, very much against my will at the time, I was detained at Arequipa (7,750) feet for a whole week, with the advantageous result that I experienced hardly any inconvenience when from there I went on to Vincocaya, at a height of 14,360 feet. I mention this in particular, as such very unfavorable reports are current regarding the railway journey on the Oroya line from Lima and the mortality among the workmen employed in constructing that very remarkable railway.

As regards the desirable instrumental equipment, there is one point which I desire to emphasize. The mirrors of the reflecting instruments should be made of speculum metal and by no means of silvered glass. Polished silver is incomparably more liable to tarnish than good speculum metal. Besides, even what would be called a good film of silver on glass is in a considerable degree transparent to ultra-violet rays, as was pointed out by Stokes and Cornu many years ago (*Annales de l'École Normale Supérieure*, ser. II, tome IX, 1880, pp. 22–23). Respecting the behavior of such a film with regard to the infra-red rays I have no knowledge, but doubtless your own Professor Langley has had abundant experience on this point. On the other hand, mirrors of speculum metal reflect the low grade heat rays of the moon, and all other rays up to the extreme known limits of the ultra-violet, with apparent equal completeness. Moreover, a mirror of speculum metal, when made of the proper alloy and well polished, is, under proper care, one of the most permanent of known optical appliances. I have before me the Cassegrain mirror of a reflecting telescope of 6 inches aperture, made by Short in 1745; both it and the other mirrors of that telescope are, to use the words of Dr. Dreyer, "as bright as if they had been polished yesterday." I must add, however, that the telescope, which formerly belonged to King George III, and is now at the Armagh Observatory, seems to have been but rarely used. We have here, however, a Gregorian reflector made by Cary something over 100 years ago, which is frequently used by us for watching the timeball, and though never repolished, is still so bright that one would hesitate to relegate it to the polisher. We have also the magnificent 5½-inch grating presented by the late Professor Rowland more than 20 years ago, also made of speculum metal, which is practically as good as when it was first received by Lord Crawford, though it has been in very frequent use. During one course of observations this grating was exposed to the fumes from peaty ground, which we

found very injurious to silver on glass, but which did not perceptibly affect the brilliancy of the surface of the grating. Doubtless you will be able to learn the exact composition of the alloy used for the Rowland gratings; but probably the combination of 4 atoms of copper with 1 atom of tin, recommended by the late Lord Rosse in his account of the construction of the great telescope at Birr Castle, would yield an alloy capable of retaining its polish for many years. From Lord Rosse's own account it seems that he himself used a somewhat softer alloy, with the consequence that the larger mirrors made by him required repolishing about once every two years; but I may mention that the night air at Birr Castle Observatory is usually very damp, and that owing to the great mass of the mirrors they are very liable to get dewed. This brings me to another part of the subject.

A serious objection to metallic specula, as usually constructed, is their great weight; but this difficulty may be largely remedied by giving the metal a more suitable form than that of a simple circular slab or disc. By the use of suitable sand (such as is used in the production of the highest class of bronze castings) there should be no difficulty in casting a speculum with deep ribs on the back, which would be much lighter and relatively stiffer than a disc of the same diameter. For a mirror 54 inches in diameter, I would suggest making the ribs and face of a uniform thickness of ½ inch. By giving the speculum a total depth of 8 inches, it would probably be stiffer than any mirror yet cast, and with a suitable arrangement of the ribs would weigh about 1,200 pounds. Of course, I assume that the "metal" would be cast face downwards on a "bed of hoops" of the proper curvature, as practiced by the late Lord Rosse, to insure that the surface to be ground and polished should be perfectly sound and almost exactly of the desired form. The possibility of casting a speculum of this shape was, in a great measure, set at rest by an experiment made by the present Lord Rosse, who cast an elliptical flat mirror some 11 inches by 8 inches with a ribbed back, some thirty years ago. This mirror was perfectly sound and took a very high polish. It was used as a diagonal mirror for the 6-foot Newtonian reflector. If I were trying the experiment I should be inclined to honeycomb the mirror after this fashion, adding a "web" round the outside, but taking care to make every part, including the face of the mirror, of the same thickness to facilitate uniform contraction in cooling. It is almost needless to say that a casting of this kind would need to be carefully annealed. Provided the

pattern were made in two parts, back and front, of cast iron and carefully finished, it is quite possible that a uniform thickness of ⅜ inch would be quite sufficient; this would reduce the weight of the finished speculum to 900 pounds. Probably the chief practical difficulty in making such a casting will be so to arrange the mold that it will readily yield to the contracting speculum metal. Very much will depend on the nature and condition of the sand or loam used in forming the mold, but doubtless valuable advice on the whole process could be obtained from an experienced molder who has been accustomed to the production of complicated and fragile castings.

In conclusion, if we regard the rapid progress in spectroscopy of late years associated with the improvement of the diffraction grating, it seems that this is probably no less due to the happy choice of speculum metal for the material of gratings than to the improved accuracy of the ruling. It is therefore reasonable that further advances in many other branches of astrophysics may be expected from a return to the use of the solid metallic reflector in place of the mirrors of silvered glass now so much in favor.

ACKNOWLEDGMENTS.

The Committee desires to acknowledge the important advice and suggestions received from Dr. Elihu Thomson, Professor Joseph N. Le Conte, Major George W. Stewart, Professor E. C. Pickering, Professor H. Rubens, Professor F. Paschen, Dr. S. W. Stratton, Mr. T. P. Lukens, Mr. James Gamble Rogers, Mr. C. A. Phillips, Mr. Wm. R. Staats, Miss A. M. Clerke, Professor H. H. Turner, Mr. John Broder, Mr. James Lyman, Dr. G. K. Gilbert, Dr. C. Hart Merriam, and others, particularly from Mr. Charles G. Abbot, Assistant in Charge of the Smithsonian Astrophysical Observatory, who, with the approval of Secretary Langley, furnished a very large amount of detailed information.

PAPERS RELATING TO GEOPHYSICS

REPORT ON GEOPHYSICS

By C. R. Van Hise,

ADVISER IN GEOPHYSICS.

CONTENTS.

In the report of the Advisory Committee for Geophysics submitted last year, the establishment of a geophysical laboratory is somewhat fully considered. This report I was asked to supplement. Before taking up additional points it may be well to briefly summarize the reasons already advanced for the establishment of such a laboratory.

Why a Geophysical Laboratory Should be Established.

In recent years there has been no more striking development than cooperation in industrial enterprises. Whatever may be thought about certain aspects of such cooperation, there is no question that from the point of view of abundant cheaply manufactured products, industrial cooperation has been of enormous advantage.

Science has reached a stage in its development in which cooperation is as essential as cooperation in business. When the sciences were young—indeed, until very recently—work was done in each in comparative independence of others. But the independent advance of the sciences has left unoccupied great intermediate fields. This

is illustrated by the rise, within the last quarter of a century, of physical chemistry and astrophysics. Van't Hoff, Ostwald, and others, seeing that there was a great unoccupied field between physics and chemistry, began the occupation of it. The great results reached by these men placed their names very high in the roll of those who have contributed fundamental ideas to science. More recently we have seen the marvelous rise of astrophysics. The scientific fruits yielded by occupying the ground between astronomy and physics have not been less important than those which have come from occupying the ground between chemistry and physics.

The purpose of a geophysical laboratory is to take possession of the vacant ground between geology and physics and geology and chemistry. So long as geology remained a descriptive science it had little need of chemistry and physics; but the time has now come when geologists are not satisfied with mere descriptions. They desire to interpret the phenomena they see in reference to their causes—in other words, under the principles of physics and chemistry. If this be done the intermediate ground between geology and physics and chemistry must be occupied. This involves cooperation between physicists, chemists, and geologists. If such cooperation be undertaken in a systematic way upon an adequate scale, it is believed that there will be a greater revolution in the science of geology than it has hitherto undergone. Instead of being a descriptive science, it will be a science reduced to order under the principles of physics and chemistry, or, more simply, under the laws of energy. It is also believed that incidentally the sciences of physics and chemistry will be enormously advanced by the investigations undertaken.

As showing the advantages which may come from the cooperation of geologists with scientists in the other branches, there may be mentioned one kind of cooperation, which has already begun upon a considerable scale, which does not necessarily require a laboratory: cooperation between geologists and mathematicians. In the past, many mathematicians have taken up geological problems, but usually their discussions have been unsatisfactory because important geological data were omitted from their premises. But by cooperation with a geologist the mathematician is enabled satisfactorily to apply his mathematics to geological problems because he has a full statement from a competent geologist as to the geological factors which enter into the problems. The mathematician publishes his results and gets full credit for his work. The geologist then applies these results to his geological problems. Thus by action and reaction

between the geologist and the mathematician rapid advance is being made in knowledge of the early history of the earth, knowledge which could not possibly have been reached by geologists alone or by mathematicians alone.

In a geophysical laboratory the geologists would cooperate with chemists and physicists in a similar manner. Expert chemists and physicists would apply chemical and physical methods to the problems of geology. The phenomena and the various conditions under which they were probably produced would so far as possible be made clear to the chemists and physicists in advance of their work. The results reached by the chemists and physicists would then enable the geologists to advance their part of the work. This would lead to further suggestions to the chemists and physicists. One man would not be working for another. The men in the different sciences would be working together for the advancement of knowledge. They would publish their results separately or jointly, as seemed best.

Thus by cooperation, action and reaction between men in the three different departments, the great field between geology and chemistry and physics would be occupied. The fundamental work for the new science of geophysics would be done.

The results which would follow from geophysical work on a large scale are believed to be at least as great as those which have come from occupying the middle ground between physics and chemistry and between astronomy and physics. If a geophysical laboratory were established at Washington, this work would be done in America.

Scope of a Geophysical Laboratory.

The Advisory Committee for Geophysics in its report of a year ago proposed a twofold plan: First, the establishment of a geophysical laboratory at Washington; and, second, cooperation with all existing institutions or men now engaged in geophysical work. These two phases of the subject will be considered in order.

Establishment of a Geophysical Laboratory at Washington.

The report of the Advisory Committee referred to discusses broadly the various problems which should be taken up in a geophysical laboratory and gives a provisional plan for the construction of such a laboratory. The time available to the committee for the

preparation of its report was short, and there were included in the report all important problems of geophysics which occurred to the committee. There was not sufficient time for consultation with geologists as to which of the problems proposed are most pressing, nor with physicists as to which of the problems experience has shown can be attacked with the certainty of securing results. My supplementary work has therefore taken these directions.

The proper construction of a geophysical laboratory is considered by Dr. G. F. Becker, who submits an independent report upon this subject.

Opinions of Geologists as to Importance of a Geophysical Laboratory.

In consulting with geologists as to the lines of work which seem to them essential for the progress of the science of geology, it has been necessary to lay before them the general project of a geophysical laboratory at Washington, so that incidentally their views have been learned as to the importance of the establishment of such a laborar tory to the progress of the sciences of geology, physics, and chemistry. Upon this matter there has been but one opinion: that the establishment of a geophysical laboratory along the lines proposed a year since by your Advisory Committee for Geophysics would be of the very greatest service to science. This view has been expressed by directors of national surveys, presidents of geological societies, presidents of national academies, and many eminent geologists. It seems unnecessary to extend this report by inserting all the statements upon this subject made by various men, but a few may be inserted by way of illustration.

Dr. J. J. H. Teall, Director General of the Geological Survey of Great Britain, says he has "no doubt that a central laboratory of geophysics in Washington, organized in the manner which is suggested in the report of the committee, would contribute very largely to the progress of science." Sir Archibald Geikie, formerly directo- of the same Survey, says in reference to the plan of the committee: "The scheme seems to me well considered and likely to lead to the most important results in the future. * * * International co-operation is destined in the future to play a large part in the progress of science, and the geophysical laboratory at Washington might be made a powerful medium for establishing and fostering this broad spirit of brotherhood in research." Professor Törnebohm, of the

Swedish Geological Survey, says: "In my opinion, the plan of establishing a geophysical laboratory is a grand one. Ably conducted, such an institution may no doubt proffer elucidation on many an obscure question and powerfully promote the progress of many branches of geology and petrology in general." Professor Sederholm, of the Geological Survey of Finland, says: "The enterprise which you hope to start aims at nothing less than to lay a new and in many respects more certain base for geological science. There can be no question about the exceedingly great advantage to the science of such experimental studies. If they have till now played an inconsiderable part in geology, it has been mostly because it has not been possible to make them on a scale in any measure adequate to that of nature." Professor Suess, President of the Royal Academy of Science in Vienna, says: "I would heartily envy the country which might first boast of such an institution."

These opinions are in accord with those expressed by leading physicists and chemists to the Secretary of the Carnegie Institution and published as an appendix to the report of the Advisory Committee for Geophysics. These men, all of whom speak of the importance of geophysics to geology, or to geology and science in general, include Poncairé, Lord Kelvin, Ernst Mach, Becke, Kohlrausch, Van't Hoff, G. H. Darwin, and Nernst.

The establishment of a geophysical laboratory was also discussed with many geologists at the International Congress of Geologists at Vienna this year, and there was but one opinion among representative geologists—that the foundation of a geophysical laboratory would do work of fundamental importance for the science of geology.

Indeed, the Council of the International Congress of Geologists unanimously adopted a statement concerning the subject which was accepted without dissent from any source by the entire Congress. This statement is as follows:

"It is a well known fact that many of the fundamental problems of geology—for example, those concerning uplift and subsidence, mountain making, vulcanology, the deformation and metamorphism of ore deposits—cannot be discussed satisfactorily because of the insufficiency of chemical and physical investigations directed to their solution. Thus, the theory of large strains, either in wholly elastic or in plastic bodies, has never been elucidated, while both chemistry and physics at temperatures above a red heat are almost virgin fields.

"Not only geology, but pure physics, chemistry, and astronomy would greatly benefit by successful researches in these directions. Such researches, however, are of extreme difficulty. They would

require great and long sustained expenditure as well as the organized cooperation of a corps of investigators. No existing university seems to be in a position to prosecute such researches on an adequate scale.

"It is therefore, in the judgment of the Council of the Congrès Géologique International, a matter of the utmost importance to the entire scientific world that some institution should found a well equipped geophysical laboratory for the study of problems of geology involving further researches in chemistry and physics."

In view of the foregoing facts, I think I may unhesitatingly assert that not only the geologists of this country, but the geologists of the world, and all the chemists and physicists who have given any attention to the subject, believe that the results which would be obtained by the establishment of a geophysical laboratory would lead to fundamental advances in the science of geology and great advances in the sciences of physics and chemistry.

The Work of a Geophysical Laboratory.

The general lines of work of a geophysical laboratory are fully set forth in the first report of the Advisory Committee. It has been my aim to supplement this part of the report by ascertaining the nature of the problems which geologists regard as most pressing and which chemists and physicists regard as capable of being successfully attacked. I am not able to make an exhaustive statement in these respects; but, as a result of many conferences, I can specify certain lines along which enough work has been done to make it certain that important results will follow from adequate investigations. While the problems here mentioned are by no means exhaustive, they are sufficiently numerous to show that there is ample work which should be taken up at once to occupy a geophysical laboratory for many years. Some of these problems are as follows:

(1) *The Relations of Liquid and Solid Rocks.*—A line of work along which many geologists are asking for information is that concerning the relations of liquid and solid rocks. They want to know the melting points of rocks, the temperatures at which rocks crystallize from magma, the relative specific gravities of melted and crystallized rocks, the effects of slow cooling upon the crystallization of rocks with and without pressure, the solution of one kind of rock in another, and, in short, all the phenomena which concern the transformation of magma to crystallized rock and of crystallized rock to magma. Upon these various points almost no information is avail-

able, and yet reliable knowledge in reference to them is necessary before the phenomena of vulcanism can be put upon a scientific basis. Experiments in laboratories on a small scale show that this work can be done. But the work has never been done upon an adequate scale, nor is there any probability that it will be done upon an adequate scale, so that the results can be applied to the history of the earth, until a well equipped geophysical laboratory is constructed with sufficient funds to operate on a large scale.

Lord Kelvin suggests that in experimental work involving many of these points at least a cubic foot of the melted rock should be taken. Among other Europeans who mention experimental work along these lines as essential are Dr. Ernest Schwarz, of the Geological Commission of the Cape of Good Hope; Professor Loewinson-Lessing, of the Polytechnic Institute of St. Petersburg; Professor Vogt, of Kristiania, and Professor Suess, of Vienna. Also the necessity for this kind of work has been especially emphasized in America by an important group of geologists, including Adams, Cross, Iddings, Kemp, Lane, Pirsson, Washington, and Wolff. Their views upon this and other pressing investigations in geophysics are set forth in a paper accompanying this report. The carefully systematized, comprehensive plan of work outlined in this paper will be of great assistance to the experimenter if a laboratory is constructed.

Sufficient work has been done by Morosiewitsch, Doelter, Brun, and others to show that an investigation of the relations of fluid and crystallized rocks will be very fruitful. In America, Professor Carl Barus, under the direction of Clarence King, once began investigations upon fluid rock, but this work was unfortunately discontinued because of lack of funds. Little work along this line is being done anywhere simply because of the lack of properly equipped laboratories with adequate funds to carry on such necessarily expensive work. If such work be provided for in a geophysical laboratory at the Carnegie Institution, no one can doubt that scientific results of the first order will be obtained.

(2) *Minerals and Rocks from Aqueous Solutions.*—Another class of investigations is the artificial production of minerals and rocks from aqueous solutions. This involves a study of natural solutions, both those of the sea and those in openings in rocks, in order to determine the conditions under which minerals crystallize from such solutions. Already the study of natural solutions with reference to the crystallization of salt and gypsum has been undertaken by Van't Hoff.

This great chemist has reached many important results, but he points out that very much remains to be done, and especially recommends this line of study to be taken up on an adequate scale in a geophysical laboratory. Experiments should be carried on with aqueous solutions under various pressures and at various temperatures. The higher temperatures should approach those of magmas, in order that the relations of crystallization from magmas and crystallization from water may be learned. It is held by some that there is gradation between these. Sufficient has been done by various workers to show that very important results can be reached by the investigations proposed, and a well organized, comprehensive series of experiments is now needed. It is certain that the conditions under which many of the minerals produced in nature from water solutions can be produced in the laboratory. Only when this is done shall we have an adequate basis upon which to judge of the kinds of minerals that are produced in nature from aqueous solutions and their manner of formation.

The study of natural underground solutions and the artificial production of minerals have a most intimate relation to ore deposits. Already studies along these lines have led to large advances in knowledge of the development of ores. This the men engaged in mining have recognized. Very recent contributions upon this subject have been of great practical importance in the exploration and exploitation of ores. There is unanimity of opinion among geologists that experimental studies on underground solutions and the artificial reproduction of the natural minerals will lead to correct theories of ore deposition and also give results of practical value, the magnitude of which cannot now be estimated.

(3) *The Deformation of Rocks.*—Elaborate experimental work should be done upon the deformation of rocks under different conditions of speed, temperature, pressure, and moisture. At the present time Dr. Frank D. Adams, at Montreal, is engaged in the slow deformation of one rock—marble—on a small scale. Indeed, in this work he has the support of the Carnegie Institution; but experiments along this line need to be carried through long periods of time for many kinds of rock on a much larger scale than heretofore, in order that the results may be applied with safety to the observed deformation of the vast masses of material of the earth. But already sufficient preliminary work has been done to show that this is a field for laboratory investigations which will certainly yield important results to the science of geology.

Dr. James Dewar, Professor of Chemistry in the Royal Institution of Great Britain, is now engaged in testing the strength of rocks at the temperature of liquid air. Already he has reached remarkable (unpublished) results; but he states that the apparatus and equipment at his command are entirely inadequate to carry on experimental work on the deformation of rocks at low temperatures on a scale that such work demands in order to give satisfactory results. He says that if a laboratory of geophysics were established the determination of the breaking strength of various rock masses, by compressive, tensile, and tortional stresses, should be made at low temperatures. He says further that a complete determination of the elastic constants of rocks at different temperatures, under stresses of various kinds, should be made. Professor Dewar states that by the low temperature work upon very small masses of a few varieties of rock in his laboratory he expects to show merely that very important results can be reached by this line of work, and thus to lay out a great field for extensive work along the same line. Such work as that proposed by Professor Dewar is not provided for anywhere in the world. Such work is especially appropriate to a geophysical laboratory.

(4) *The Constants of Rocks.*—Another set of problems which many geologists desire attacked concern the constants of rocks at various temperatures and pressures, such as their densities, their coefficients of expansion, their specific heats, conductivities, etc. The lack of knowledge of these constants, which can certainly be determined by experiment, has stood in the way of the progress of geology in various directions. The need for work along these lines is especially emphasized by Lord Kelvin and Professor Dewar, and is discussed by Dr. Becker in his report of last year.

Basis of Selection of Problems Suggested.

In mentioning the foregoing broad lines of investigation I have confined my statements to those which are urgently demanded by many geologists as necessary for the progress of the science. They represent the consensus of opinion of the many geologists with whom I have conferred rather than my own views. I have purposely omitted the problems mentioned in the first report of the Advisory Committee for Geophysics that are somewhat more remote from the present pressing problems of the geologist and the student of ore deposits.

In order to recall some of the lines of work which are not here

considered, it may be said that all of the great problems concerning the atmosphere set forth in the first report of the committee are wholly omitted; also the great problems dealing with the interior of the earth have been ignored. Finally, all the problems along the border line of astronomy and geology which concern the early history of the earth have been omitted. By these omissions I do not mean to imply that each of the lines is not of profound importance. Indeed, I believe that all should ultimately be taken up in a geophysical laboratory. It may be taken for granted that a deeper insight into the order of the universe is a sufficient reason—indeed, is the most important and fundamental reason—for investigations in science. All of these omitted lines fall within this class of studies, but the report of the Advisory Committee for Geophysics has already fully covered these problems. A special purpose of this supplementary report is to emphasize the point that there are many problems of immediate importance to the science of geology and to a knowledge of ore deposits which deal with the part of the earth that we can see, concerning which experimental work is demanded by the geologists of the world, because lack of such work stands in the way of the advance of science.

Cooperation in Geophysical Work.

I shall next consider the second part of the proposal of the Advisory Committee of last year—that of cooperation in geophysics. The plan of the Advisory Committee provided for the use of branch laboratories in various parts of the world. It was thought it might be necessary to construct an occasional small branch laboratory, but, so far as possible, it was proposed that all existing laboratories should be utilized to the fullest extent; also the plan of cooperation provided that the central laboratory at Washington should be a clearing-house for the geophysical work of the world. This clearing house would acquire accurate information as to the geophysical work being done in all laboratories of every country. Any scientist who wished to know the present status of knowledge in reference to any problem and what others are doing, so that he might take up work which should not duplicate that already done or being done by others, could apply to the Carnegie Institution at Washington and obtain the needed information.

No part of the general plan of the Advisory Committee has received more universal approval by geologists, physicists, and

chemists of various countries than its proposal for cooperation in geophysics.

BRANCH LABORATORIES.

It has been suggested, especially by Professor Loewinson-Lessing, that a branch laboratory in the Hawaiian Islands, which are now a possession of the United States, would give unexampled opportunities for the study of vulcanism. The majority of the present living volcanoes are comparatively small. In Hawaii are the greatest of the existing volcanoes—those that are most nearly comparable to the ones which must have existed when the vast lava plateaus of various parts of the world were produced. If a branch laboratory were established in Hawaii, there can be no question that the knowledge of the phenomena and causes of vulcanism would be greatly advanced.

SEISMOLOGY.

Another line along which cooperation is especially urged by various European geologists is seismology. It is unnecessary to urge the importance of seismological investigations both to science and to constructional work. At the present time there are many seismological stations scattered over various parts of the world. However, for an adequate study of earth tremors it is advisable that additional stations should be established at a number of wisely selected places in the more remote parts of the earth. Professor Milne at Shide, Isle of Wight, has for many years been receiving records of a large number of the instruments now in use, but the work has now become too large for him to carry, and he asks for assistance. Recently it was arranged that Strassburg be a center of information for seismology, but some countries have refused to cooperate in this plan. The time is now ripe for some institution with adequate funds to arrange a broad scheme of cooperation between the various interests and to be the medium which harmonizes them, and thus to systematize the seismological work of the world. Many have said to me that the unique position of the Carnegie Institution, free from all entanglements and prejudices, places this institution in by far the most advantageous situation to accomplish this work. Indeed, a number of geologists have said that, so far as they can see, unless the Carnegie Institution takes up this work, the same chaotic condition of affairs that has existed in the past will continue. It is believed that in securing the cooperation of all the men engaged in

seismology and in coordinating all of the work on seismology, a laboratory of geophysics at Washington would find one of its greatest opportunities.

Cost.

As to the cost of a geophysical laboratory, the committee of last year submitted an estimate which it thought sufficient to provide adequately for the great plans laid out by it. If the scope of the proposed laboratory be confined to the more pressing lines of work indicated in this supplementary report and to other problems of an equally pressing character, and the more remote problems are ignored for the present, the cost of a laboratory can be very considerably reduced. Indeed, to get very important results, it is not necessary that all of the problems discussed in this report shall be taken up at once. If a laboratory were established, the governing body could best decide which problems should be undertaken after it was known how much money was available. After deliberate consideration of the matter from the minimum point of view, rather than from the point of view of what is desirable, I have come to the conclusion that work of very great importance can be done in geophysics for $50,000 per annum; but in order not to greatly delay work, it is strongly urged that $100,000 be appropriated toward a building. This would make it possible to begin work on a productive scale much sooner than if only $50,000 per annum were appropriated, and from this fund it were necessary to construct the building and purchase apparatus.

My recommendation is, therefore, that there be appropriated for the construction and maintenance of a geophysical laboratory $100,000 and $50,000, with the expectation that the latter appropriation will be an annual one.

While with the amount suggested it will not be possible to press the various lines of geophysical work with the speed which many strongly hold to be exceedingly desirable, I feel confident that great, indeed revolutionary, results to the science of geology will be obtained.

CONSTRUCTION OF GEOPHYSICAL LABORATORY

REPORT BY GEORGE F. BECKER.

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ORIGIN OF REPORT.

In March, 1903, Mr. Walcott requested me to cooperate with Professor Van Hise in gathering information abroad with reference to the construction suitable for a geophysical laboratory, and in regard to the problems which could be profitably studied in such a laboratory were it to be built. After consultation with Professor Van Hise it was decided that the most important features of laboratories are the means adopted to secure stability of piers and the methods of obtaining constant temperatures. Laboratory construction must determine in what measure stability of instruments and constancy of temperature can be attained. On the other hand, to the investigators who occupy the laboratory after completion must be left in large measure the details of apparatus and of methods of research.

Laboratory construction is a matter of extreme importance and one which has been, relatively speaking, neglected. Vast ingenuity has been applied to the perfection of apparatus, while little pains has been taken to provide for that freedom from mechanical and thermal disturbance without which many instruments of precision cannot possibly give the best results of which they are capable. Hence also the work done in an ill constructed laboratory, other things being equal, will be inferior in quantity and quality as compared with that achieved in a suitable building.

Institutions Visited.

In accordance with the plans thus laid, I visited the Cavendish laboratory at Cambridge, the laboratory of the Sorbonne and the astronomical observatory at Paris, the laboratory of the Bureau International des Poids et Mésures at Sèvres, the physical laboratories of the universities of Strasburg and Würzburg, the geophysical laboratory of Göttingen, the laboratories of the Physikalisch-Technische Reichsanstalt of Charlottenburg, the laboratory of the Normal Aichungs Komission of the same place, the Astrophysical observatory of Potsdam, the physical laboratory of the University of Leipzig, the cryogenic laboratory, and the Astronomical observatory of Leiden.

So far as possible, I consulted the chief physicists or astronomers of these institutions on the main points of my inquiry. Professor J. J. Thomson was absent and Professor Onnes, of the cryogenic laboratory, was engaged on the day of my visit. On the other hand, I had instructive interviews with Professor George H. Darwin, Professor Charles Chree, Sir Archibald Geikie, Professor E. Suess, and others.

Nearly everywhere I have found physicists dissatisfied with the construction of their laboratories and fully persuaded that radical improvements are possible. Satisfactorily firm piers have been constructed only at Potsdam and Leiden, and in both these places the successful result seems due rather to natural conditions than to peculiarities of construction. Fairly uniform temperatures, excepting in underground chambers, have not been attained, although a majority of physicists are of opinion that they might be brought about. Heating and ventilation are usually no better than in any ordinary office building.

An ideal laboratory would be free from magnetic, electrical, or mechanical disturbances and from unintentional changes of temper-

ature. It would be possible to maintain any room at any desired temperature for any desired period of time consistent with good lighting and ventilation. Such conditions cannot be fully realized.

Magnetic Disturbances.

Magnetic observations are so subject to disturbances that in practice it is found needful to provide for them in separate buildings, free from iron and as remote as possible from industrial establishments. For more general laboratories, therefore, purely magnetic disturbances may be left out of account, and iron may be freely employed in construction so far as it does not lend itself to the propagation of mechanical vibrations.

Electrical Disturbances.

Electrical disturbances are of two orders of magnitude: Trolley lines using an earth current produce serious electrical disturbances at a distance of at least one mile, while trolley lines with a double overhead or underground metallic circuit, as well as carriages deriving power from storage batteries, are innocuous at a distance of only a few hundred feet.

Two Desirable Constant Temperatures.

Except in deep subterranean chambers, it is difficult at best to maintain uniform temperatures. Far greater is the difficulty of changing the temperature of a room at will, for a very long time must elapse before the massive masonry of the walls and piers acquires the new temperature. For these reasons it appears inexpedient to attempt more than two temperatures in any laboratory, except perhaps in one small room. One of these temperatures is the mean temperature of the subsoil, say 9° or 10° C. in temperate latitudes, and the other is a comfortable temperature for work, say 20° C.

Annual Mean Temperature.

The maintenance of the lower temperature with extremely slow variations of a few degrees is not difficult in cellar or subcellar spaces. It is also possible, as Professor Wiechert has shown, to keep the air in such spaces moderately dry. Mr. Wiechert admits the air to his seismometer house through a galvanized-iron tube,

which is convoluted between the double walls of the house and provided with drips. The tube enters the inner chamber near the ceiling and passes round the entire inner space, always at a slight slope, so that all condensed moisture trickles backward. From the air tube air escapes into the instrument room through small holes in the side of the tube. The result is, for some purposes, a very satisfactory one, attained almost without expense. In a larger laboratory a somewhat different method would probably be more convenient and more effective.

Difficulties of Varying Temperatures.

Attempts have been made in Europe to control the temperature of large apartments by providing them with double metallic walls in which hot or cold solutions circulate, but these efforts have not been successful. In warm weather, when cold solutions must be employed, the walls drip with moisture, the instruments suffer, and the operators fall ill. This would be avoided by supplying the apartment with air not merely cooled but dried, just as a room in winter may be heated by a hot-air furnace.

In this country more attention has been given than in Europe to cooling apartments with dry air. The Bureau of Standards has developed arrangements for this purpose which will be in operation in a few weeks, and it is said that the Stock Exchange in New York is being similarly equipped. The experience obtained by the Bureau of Standards should be carefully considered before any specific plan is adopted for a geophysical laboratory.

Importance of Uniform Temperatures.

In my opinion, a modern laboratory should be supplied in summer with dry, cool air, the temperature of which is under control. Such air of appropriate temperature should be admitted to the cold subterranean chambers when required, and should be furnished to the ordinary laboratories in such quantity as to keep their temperature down to 20° C. = 68° F. In winter the rooms must, as a matter of course, be warmed. If the problem of maintaining a laboratory at constant temperature is not wholly simple, it is surely of small complexity as compared with those of physical research, and it cannot be doubted that were its solution requisite to the success of a commercial enterprise, an appropriate method would soon be developed. Yet it is unquestionable that physical research would

proceed much more rapidly and effectively in a laboratory of fairly constant temperature. Some physicists, indeed, maintain that it is sufficient to attempt constant temperatures only within pieces of apparatus, but in this view I cannot agree. A standard bar, for example, may be measured in a case kept nearly at constant temperature by circulating liquids or by electric resistance; but this temperature depends in part on the radiation of the case, and this on the temperature of the apartment. Again, the accuracy of galvanometers, and all similar apparatus, is greatly promoted by a substantially uniform temperature.

Special Difficulties in America.

In the eastern United States the natural atmospheric temperature varies so enormously and so rapidly as greatly to interfere both with the accuracy of instruments and the capacity of observers. Heat flows from piers to instruments, or in the reverse direction, so fast as to be most disturbing and wholly incalculable, and this flux renders the more minute measurements most uncertain. Thus, even more than in Europe, it is desirable that an American laboratory of the highest class should be isothermic.

Avoidance of Heat Flux.

The maintenance of a temperature of 20° C. in a laboratory is attended with other difficulties besides that of supplying cool or warm air. In the laboratory of the Normal Aichungs Kommission it has been found that the flow of heat downward through the piers is a very disturbing factor at the best, and I there received the excellent suggestion that the exposed portion of the piers should be made of metal, in order that it should readily take on the temperature of the observing room. The metal plate should stand on three points, and the stratum underlying them should be of the most non-conductive material which can be found. Hard magnesia brick is almost ideal in this respect, and I suggest that the masonry of the piers be faced with this material.

Notes on Ventilation.

Insulation, excepting in very deep subcellars, is not sufficient to establish uniformity of temperature, even in Europe. In the eastern United States, the annual variation being much greater than on the other side of the Atlantic, insulation is still less effective.

Modern American engineers have reached the conclusion that either in warming or cooling apartments diffusion of the air, unaided by stirring, cannot be relied upon to produce substantial uniformity of temperature. Air of nearly the desired temperature must be forced to circulate through the room at velocities which are sensible, but are not necessarily great enough to constitute deleterious drafts. In my opinion, cold air should be admitted in summer at numerous openings near the ceiling of rooms, while heating should be effected in winter by warm air entering at many openings in or near the floor. Ventilation should not be left to natural draft alone, for this sometimes fails; it should always be possible to control it by electric fans or other engines. Thermostats should be employed, but they should be of very solid and durable construction, and they should be carefully checked until found entirely satisfactory.

Suggestion that Vibration of Piers be Damped.

The subject of stable piers is one of the most vital importance to laboratory construction, yet it has been most imperfectly investigated. There is no question whatever that superficial vibrations of the soil are largely cut off by a trench excavated about a pier, and for this reason the whole new laboratory at Leipzig is inclosed in a trench. On the other hand, the base of a pier under ordinary conditions is its stablest portion, so that the top of a tall pier vibrates far more than its base. It occurred to me that the vibrations of a tall pier might be damped, for example, by filling the trench about it with coarsely ground cork or some similar substance. I seemed to see an illustration of this principle in Wiechert's seismometer, an instrument which is, of course, intended to respond to the most minute vibrations. Except for a very essential device, this seismometer, when once agitated, would continue to vibrate for a long time with a period of its own. This would, of course, defeat its purpose. To render it a "dead-beat" instrument, it is damped by air cataracts, and thus records only the tremors communicated to it by the earth. Now, why should a pier not also be damped by air cataracts, cork-packing, or other means? In Leiden I met what seems a most surprising confirmation of this idea. The Cryogenic Laboratory and the astronomical observatory are built on soil which overlies some 40 feet of soft mud resting on sand and clay. This seems a most unpromising position for stability, yet experience has shown that it is not so. The piers are built on groups of piles driven well down into the

sand and sheathed in planks. The buildings are built on systems of piles inclosing those of the piers. Now these piers are so stable that in the Cryogenic Laboratory observations with the most delicate galvanometers are entirely practicable when five condensing pumps are at work in a room only a few yards distant, while in the observatory there is not the least trouble in using the quicksilver mirror.

On the other hand, at the Bureau des Poids et Mésures wagons on a high road some hundreds of yards away shake the piers, and the quicksilver mirror is seriously disturbed even at the bottom of the catacombs near the Paris Observatory. It would seem to me that the mud underlying Leiden damps the vibrations of the piers much as cataracts would do, and that this is the only probable explanation of their success, which I understand to have been attained without special reference to the efficacy of mud for this purpose.

Research Called for.

The subject is one needing and deserving research. In the well-known Julius suspension, means are adopted both to secure damping and to place the instrument in a node of vibration. The extraordinary efficacy of this suspension is well known. It appears to me perfectly possible to devise piers, after proper investigation, in which vibrations will likewise be damped and in which the upper surface will lie in a node.

While the results in Leiden show that mud underlain by firmer material is not a bad foundation for a laboratory, no one, I take it, would deliberately select such a situation if solid rock or a firm saprolite (decomposed rock in place) were available.

Construction of Laboratory Building.

All the most delicate experiments of a laboratory would be carried on on piers and in the basement or the first floor of the building, but great stability sufficient for a large class of experiments can be obtained in a second floor by the use of masonry arches. For this reason I cannot recommend for a laboratory the steel beam construction now used in ordinary buildings. In such buildings the oscillations due to wind would be very sensible, and any jars due to moving apparatus or similar causes would be communicated to other portions of the building much more readily than in an arched construction. Professor Wiechert has measured quantitatively the deflection of his main laboratory building by the wind.

I find physicists most positively of the opinion that the walls of laboratories should be broken by as few openings as possible. No flues for ventilation or any other purpose should be included in the thickness of the walls. Plumbing and piping should be placed in wells reserved for the purpose and conveniently accessible.

As material for the construction of laboratories, nothing seems preferable to good brick well laid. Experiments long ago made by officers of the Coast and Geodetic Survey show that sandstone should be rigidly excluded from every portion of the building. I know of no objection to concrete; provided, however, that the stone used be sharply angular and on no account consist of rounded pebbles.

The walls of a laboratory should be very thick and massive, not less than three feet in the lower story. All doors and windows should be double. It is the habit to inclose the constant-temperature rooms in double walls, but I believe that a single wall surrounded by cork brick would be equally good. This admirable material is much used as a non-conductor; for instance, in the insulation of the clocks of the Paris observatory.

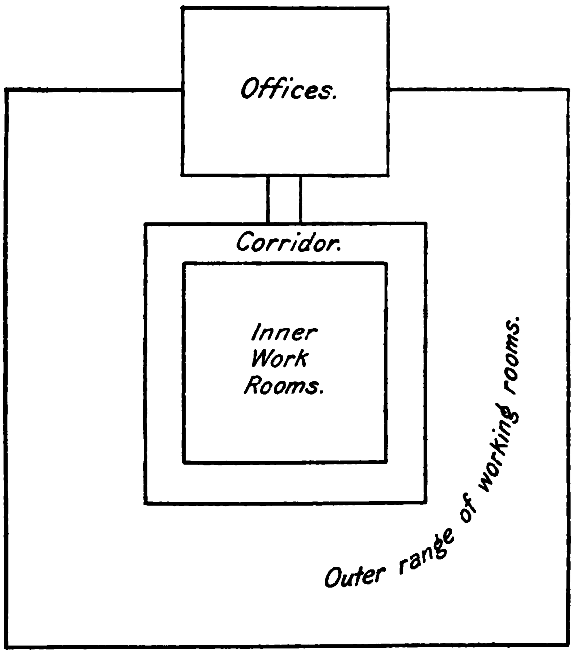
Subdivisibility.—An important feature of laboratory construction is the subdivisibility of the work rooms. As investigations succeed one another, rooms of different dimensions become desirable. The smallest room requires one window and one door, with independent gas, water, and electrical connections. So far as possible, it should be arranged that such rooms may be separated from one another by partitions of hollow brick, which can be removed without interfering with the structure of the building. Thus the size of the work rooms can be reduced to a minimum or enlarged as required at a trifling expense.

Basement Work Rooms.—The basement of a laboratory should be of sufficient height to be used conveniently as working space. In a well drained locality it affords the very best of working rooms.

Interior Work Rooms.—Opinions differ somewhat as to the best general plan for a laboratory. A few physicists object to internal rooms lighted only from above, considering them gloomy and subject to accident from the breaking of skylights. On the other hand, such rooms afford the best constant-temperature spaces and are freest from mechanical disturbance. Again, so-called wire glass and other simple expedients give ample protection from danger of falling glass. For these reasons a majority of physicists approve of internal rooms surrounded by a corridor, and this again inclosed by outer working rooms. The corridor, among other advantages, affords

most desirable space for placing cases in which to store apparatus and supplies.

In my opinion, the offices of a laboratory which are to be frequented by outsiders should be partially isolated from the main building, as indicated in the following rough diagram, so that doors leading into the main building should be opened only when this is unavoidable.



Number of Stories.—Three stories seem necessary and sufficient for a laboratory. The lowest, or basement, story should be on the ground, with one or two deep subterranean chambers beneath for secular experiments. The second story should be on arches above the basement, and would afford excellent working rooms with piers of fair stability. A third story would serve to protect the second and can be usefully employed for photographic work and experiments not dependent upon piers.

Estimates of Expense.—Estimates of the expense of building a laboratory were given in the project submitted by me to the Institution nearly a year since. These were based on the cost of the laboratory of the Reichsanstalt, and were set down at $250,000. I know of no way to improve upon this result except by having an architect draw

preliminary plans for a building. The heavy walls and arches would be expensive, but the absence of flues and the flat wall surfaces called for would tend to keep down the cost.

PROBLEMS OF GEOPHYSICS.

I have consulted Messrs. George H. Darwin, Charles Chree, Kohlrausch, and others on the problems which can be attacked in a geophysical laboratory with fair prospects of success. They appear to agree with the views set forth in Appendix 1 to Report of Advisory Committee on Geophysics (Carnegie Institution Year Book, No. 1, pp. 44–58). In particular, Professor Chree, who is the leading expert on the theory of the vibrations of a sphere, agrees with me that some years of mathematical work must be done before seismological observations can be duly interpreted. Such mathematical investigation should certainly be undertaken as soon as possible. Investigations into finite stress and strain, rupture, the steady flow of heat, diffusivity, aqueo-igneous and dry fusion, together with the whole chemistry and physics of high temperatures, afford brilliant prospects of important results and wide applications of the conclusions drawn to terrestrial problems.

It would be easy to enumerate many specific problems, and this I have done in a manuscript report to Mr. Walcott, entitled "Remarks on Geophysics," dated March 21, 1902 (not published), but the work indicated in the last paragraph would worthily occupy any institution for many years, and a consideration of what should be undertaken when this is done may well be postponed.

WASHINGTON, D. C., *October 2, 1903.*

GEOPHYSICAL INVESTIGATIONS SUGGESTED

In the interests of the Science of Petrology several of us, who have devoted much of our time and energies to petrological matters, call attention to a statement of some of the problems in the line of physico-chemical investigation which could be undertaken in a properly equipped laboratory and urge the importance of laying the matter before the geological committee of the Carnegie Institution in order that it may be favorably inclined toward the investigations proposed, some of which are already being carried on under the patronage of the Carnegie Institution.

It will be noted that many of them are of far-reaching importance to the advancement of general geological problems, of which the knowledge of the properties and history of rocks forms a very considerable part, the problems connected with rocks affecting the whole geological system. It therefore happens that problems stated in this connection may naturally be restated in others.

INVESTIGATION OF IGNEOUS ROCKS.

I. Determination of Changes of Condition Accompanying Changes of Temperature.

A. *With normal atmospheric pressure.*

1. Change of volume and of viscosity; that is, the rigidity, melting point, and liquidity of—
   - (*a*) *Rock glasses* of known chemical composition, corresponding to known igneous rocks.
   - (*b*) *Glasses* of single minerals.
   - (*c*) *Crystals* of single minerals.

This involves fusion in open crucibles with the determination of temperature thermo-electrically by methods employed by Dr. Barus and Dr. Day in the laboratory of the U. S. Geological Survey.

B. *With high pressures in closed vessels.*

The effect of increasing pressure on the volume and the viscosity—rigidity, melting point, liquidity—of *rock glasses*, and *glasses and crystals* of known *minerals*.

II. Determination of the Behavior of Solutions of Minerals in One Another.

A. *With normal atmospheric pressure.*

1. The temperature at which two minerals will go into solution in one another, and the temperature of solidification of the mixed solution.

   For various pairs of rock-making minerals such as—
   Quartz and orthoclase.
   Orthoclase and albite.
   Orthoclase and pyroxene.
   Pyroxene and olivine.
2. The point of saturation in each case for various temperatures.
3. The solution of gases in liquid minerals and rocks.
   (*a*) The rate of absorption (see also Diffusion, heading III) and the limit of saturation for different temperatures.
   (*b*) The relation between the composition of the liquid mineral or rock and the limit of saturation.
   (*c*) The effect of dissolved gases on the viscosity of the liquid rocks.

B. *With high pressure in closed vessels.*

The influence of increasing pressure on the solution of various minerals in one another, on the saturation and solidifying point, and on the solution of gases in the liquid rock and on the limit of saturation, especially the effect of dissolved gases in liquid rock under high pressure on the viscosity of the liquid. (See Diffusion, below.)

III. Determination of the Rates of Diffusion in Liquid Rocks and Minerals.

A. *With normal atmospheric pressure.*

1. The rate of *heat* conductivity in solid and liquid rocks and minerals at various temperatures.
2. The rate of absorption and transmission of various gases in liquid rocks of different compositions.

3. The rate of molecular diffusion in liquid rocks by osmotic pressure. The solution and diffusion of molecules of one kind of mineral in liquid rocks or minerals of other kinds.

   This has a direct bearing on the solution and diffusion of rocks in molten magmas, and on the theory of rock synthesis, and on that of magmatic intrusion by solution of the surrounding rocks.

4. The recognition of possible colloids in liquid rocks, which may form in the presence of gases, such as water gas, under pressure; the colloidal condition of silicon hydroxide, aluminum hydroxide, and ferric hydroxide being easily conceivable. It is also possible that more complex, alumino-silicate molecules may become colloidal. This might be detected electrolitically in molten magmas in the presence of water under pressure.

   The bearing of this on the problem of differential diffusion and differentiation is important on account of its bearing on the question of the origin of different kinds of igneous rocks.

B. *With high pressure in closed vessels.*

1. The effect of increasing pressure on heat conductivity in solid and liquid rocks.
2. The effect of increasing pressure on the diffusion of gases in liquid rocks.
3. The effect of increasing pressure on molecular diffusion by osmotic pressure.
4. The rate of molecular diffusivity at high temperatures and high pressures in liquid rocks containing various amounts of gases.

   This has an important bearing on the probably high rate of differentiation in the more liquid molten magmas.

IV. Crystallization from Liquid Rocks with Temperature Determinations and Observation of the Time Element.

A. *With normal atmospheric pressure.*

1. The determination of the temperature of saturation and the rate of crystallization of *simple minerals* in cooling liquids of the same composition.

Open crucibles.—The production of crystals of quartz, orthoclase, albite, as well as those of augite, olivine, anorthite, etc.

2. The crystallization of several heteromorphous minerals from cooling mixed solutions of the same. Examples:
   Anorthite and olivine.
   Leucite and diopside.
   (*a*) The order of crystallization related to the relative amounts of the two mixed minerals.
   (*b*) The rate of crystallization. } In the same relations.
   (*c*) The habit of the crystals. }
3. The crystallization of isomorphous minerals from mixed solutions. Such as—
   The series of albite—anorthite feldspars.
   The series of pyroxenes.
   The series of amphiboles.
   (*a*) The order of crystallization.
   (*b*) The production of distinct kinds, or of crystals of average composition. That is, the production of zonally different crystals, such as the lime-soda feldspars in many cases, or of a homogeneous crystal of intermediate composition. The relation between these modes of crystallization and the *rate* of crystallization.

B. *With high pressure in closed vessels.*

1. The effect of the presence of dissolved gases on the crystallization of anhydrous crystals from liquid rocks.
2. The effect of dissolved gases on the crystallization of—
   (*a*) Hornblende as opposed to pyroxene.
   (*b*) Biotite as opposed to orthoclase and hypersthene, etc.
3. The relation between pressure and the chemical character of the minerals or salts crystallizing from a mixed solution.
4. The possible crystallization of hydrous minerals such as epidote, analcite, muscovite, from liquid rocks under pressure (to account for "primary" epidote and analcite in igneous rocks).
5. The possible crystallization of calcite and other carbonates from liquid rocks.

6. The relation between the *size of crystals* and—
   (*a*) The *composition* of the mineral liquid—the solvent.
   (*b*) The *rate of cooling*.
   (*c*) The *mobility* of the molten magma, and its content of gas.
   (*d*) The pressure.
7. The relation between the *habit* (shape) of crystals and–
   (*a*) The *composition* of the mother liquor.
   (*b*) The *rate* of growth.
   (*c*) The molecular *mobility* of the mother liquor.
   (*d*) The absence or presence of currents or motion at the time of crystallization.
   (*e*) The pressure.
8. The *texture* of rocks in its relation to—
   (*a*) The *development of phenocrysts* by partial crystallization of the magma at one rate and the solidification of the remainder at another rate (to be effected by change of physical environment, change of temperature, or pressure).
   (*b*) The diverse rates of growth of diverse minerals in mixed solutions.
   (*c*) Synchronous intergrowths of mixed salts, as of quartz and orthoclase.
   (*d*) The effect of localized "crystallizers" in producing centers of crystallization resulting in spherulites, segregations, etc.
   (*e*) The possible development of "protoclastic" texture in moving crystallizing magma.

The physical investigations should be accompanied by close chemical studies. The precise chemical composition of all material experimented on should be determined.

The rôle played by certain elements in minerals should be investigated, such as the possibility of Al_2O_3 behaving as an acid radical in the feldspars, etc.

The molecular constitution of the more complex silicates may be investigated by means of the determination of the specific heat of the minerals.

INVESTIGATION OF METAMORPHIC ROCKS.

I. *Determination of heat conductivity in solid rocks and crystals.*

II. *Porosity of rocks.*
  (*a*) With regard to the transmission and circulation of water.
  (*b*) With regard to the transmission of gases.

III. *Solubility of rock-making minerals in water.*
  (*a*) At various temperatures.
  (*b*) At various pressures.

IV. *Solubility of rock-making minerals in vapors.*
  (*a*) At various temperatures.
  (*b*) At various pressures.
  (*c*) Also in mixtures of water and gases of various kinds, dilute acids.

V. *Solution and recrystallization.*
  Chemical reactions at various temperatures and pressures.
    Examples: *Wollastonite* converted to *calcite*.
    Calcite converted to *wollastonite*.

VI. *Physical changes accompanying hydration and dehydration.*

VII. *Growth of large crystals at the expense of small ones under static conditions.*
  The development of large, pseudoporphyritic crystals, ottrelite, staurolite, garnet, etc., in certain schists.

VIII. *Solution and recrystallization of strained crystals of rock-making minerals, and of other salts.*

IX. *Changes resulting from differential stresses.*
  1. Determination of the relation between the *rate* of deformation and the strength of the deformed rock.
  2. Relation between the amount of deformation and the degree of pressure producing it.
  3. The resistance offered by rocks to deformation.
  4. The deformation of hot rocks in the presence of water.
  5. The relative degrees of deformation experienced by given rocks at different temperatures and with different content of water, for different pressures, with limestone, marble, sandstone, granite, etc.

6. The possible production of gneissic texture in granite, and in basic rocks.
7. The investigation of the possible recrystallization of gypsum under stresses with varying conditions of temperature and moisture.
8. The same with respect to ice.
9. The comparison of the original structure and the structure resulting from deformation in the case of rocks with those seen under similar circumstances in the case of metals and alloys.
10. Theories of the effects of mutual compression on rocks of dissimilar character and diverse grades of rigidity as bearing on the dynamics of mountain-making.

X. *Effects of motion* (flowage) *on crystallization.*

1. Relation of the arrangement of primary material in bands, stratification, to the development of foliation, etc.
2. Experiments on the production of secondary foliation in rocks already foliated.

XI. *Determination of the possible relation between chemical affinities and stress.*

XII. *Determination as to the formation of graphite in metamorphic rocks,* whether it requires the presence of organic material under conditions of pressure metamorphism.

There are, of course, numerous other lines of investigation, as well as elaborations of those here suggested, which may be carried on to the substantial advancement of our knowledge of rocks and minerals and of the consequent physical character of the earth as a whole.

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JOHN E. WOLFF.

OCTOBER 1, 1903.

PROPOSED INTERNATIONAL MAGNETIC BUREAU

By L. A. Bauer.

CONTENTS.

Purpose.

The purpose of the proposed Bureau is to investigate such problems of world wide interest as relate to the magnetic and electric condition of the earth and its atmosphere, not specifically the subject of inquiry of any one country, but of international concern and benefit.

Some such problems are:

(*a*) *A magnetic survey of ocean areas and unexplored regions.*—A problem of immediate importance in view of the extensive magnetic work now being carried on in many countries and the various Arctic and Antarctic expeditions in order to complete the magnetic survey of the globe, and thus render possible the construction of more accurate and comprehensive charts. When the great part played by the magnetic needle in navigation and exploration is considered, and the intimate relationship between terrestrial magnetism and other sciences, such as meteorology, geology, and astronomy, is so called

to mind, it is unnecessary to dwell at greater length upon the practical and theoretical importance of this problem. No magnetic data have been obtained on the ocean areas since the advent of iron ships, except from occasional expeditions. Our present lines of equal magnetic declination over these waters depend almost entirely upon the data acquired in wooden ships, fifty to one hundred years ago.

(*b*) *International observations of the variations.*—An exhaustive study of the numerous variations of the earth's magnetism and electricity can be successfully pursued only by international co-operation under the direction of a central or international bureau, so as to secure uniformity in observation and reduction, and to insure the prompt coordination and publication of results. All past international work, as, for example, that conducted by many nations during the international "polar year" of 1882–'83, has suffered from the lack of a central or head bureau. During that year thousands of observations were made; the full utilization of those has not yet taken place, although they were made twenty years ago. Again, the remarkable activity now manifested over the entire globe is not being guided by any one internationally recognized bureau; we may therefore expect that, after the observations are made, it will be many years before the results of this vast amount of work will be known. Printed international forms for guidance in observation, reduction, and publication would prevent much waste of effort.

Under this heading is also embraced the establishment of secular variation or repeat stations throughout the globe, at which magnetic observations would be repeated at regular intervals, and thus make it possible to obtain the data necessary to keep magnetic maps ever up to date in all regions. The desultory way in which such observations have been made in the past has resulted in an irrecoverable loss to science.

(*c*) *Observations in ocean depths and atmospheric regions.*—As long as observations are confined to the surface of the earth, the actual distribution of its magnetism and electricity will never be known. The unequivocal solution of the problem demands observations in the waters below and in the air above. The first step consists in devising the proper instruments.

Other problems, such as the correlation of local and regional disturbances of the magnetic needle with geological and physiographic features, and the correlation of magnetic and electric disturbances with meteorological phenomena, might be mentioned.

However, sufficient has been given to reveal the promising field of research which would be opened up to this bureau, in the devising of new and more accurate instrumental means, *in the inauguration of important work in regions unoccupied by any one nation*, in the unification of methods of observation and discussion, in the coordination and correlation of observed data, and in the investigations of problems, not alone of great practical value, but also of great theoretic interest.

Method of Work.

The best means of accomplishing the purposes of the proposed bureau would seem to be through a board of international councilors, whose advice and opinions will govern the inauguration and conduct of a proposed investigation and the appointment of the proper investigators or observers. Much of the observational and experimental work will often be most effectively and economically done by furnishing means to those who have already the problem in hand. The granting of funds to such persons would be made with the assistance of the international board.

Organization.

A director, whose function will consist in the planning and general supervision of the work to be undertaken, assisted by the international council.

An assistant director, who will have immediate charge of the office work and take the place of the director when necessary.

Physicists, observers, computers, and clerical assistance as needed.

Funds Required.

An annual appropriation of $25,000, if it be made available each year, until expended, for a period of ten to fifteen years, would suffice to conduct successfully for the period the work of the bureau as outlined. However, a smaller sum, say $10,000 annually, would be sufficient to cover the expenses involved in the main function of the bureau, viz., that of direction, suggestion, devising of new instruments and methods, and prompt utilization and publication of results. A curtailment of allotment would simply imply abridgment of the experimental and observational work.

Much of the machinery required is already, to some extent, in operation, owing to the influence exerted by the international

journal, "Terrestrial Magnetism and Atmospheric Electricity," which is in touch with every prominent investigator in its special field of inquiry.

Letters Regarding the Project.

[*Mr. O. H. Tittmann, Superintendent of U. S. Coast and Geodetic Survey, to the Carnegie Institution.*]

It is my privilege to forward to the Carnegie Institution a project proposed by Dr. L. A. Bauer for an International or Central Magnetic Research Bureau, for the investigation of problems of the earth's magnetism, of world-wide interest. For want of such an international bureau many of the benefits hoped for from magnetic work, it appears, have not been realized, primarily, because of lack of unification, correlation, and prompt publication of results.

The establishment of a bureau such as proposed has been agitated at various times, and especially of late, by eminent German magneticians, such as Professor Neumayer, Director of the Deutsche Seewarte, Hamburg, and Professor Adolf Schmidt, of Gotha, both leading authorities. Its founding, in connection with an International Research Institution, which I understand is the purpose of the Carnegie Institution, would seem to me most appropriate, and would avoid the prime difficulty likely to be encountered in securing the hearty and harmonious cooperation of all nations, were the Bureau established under the governmental auspices of any one country.

No nation at present can wield a wider influence in magnetic work than our own, because of the extent of territory under our jurisdiction and because of the fact that all magnetic work is being carried out according to a common plan under one organization—the Coast and Geodetic Survey. This influence would be immeasurably increased by the founding in our country of the proposed International Magnetic Bureau. As evidence of the hearty cooperation of all nations that it is possible for us to secure, I desire to call your attention to the success achieved in its work and purposes by the international journal, "Terrestrial Magnetism and Atmospheric Electricity," founded in this country.

The plan proposed is, in brief, to employ the necessary number of persons at the main office at Washington, and to conduct research work by granting funds to the best qualified persons in any country,

the results of such research to be published under the name of the Carnegie Institution, the Institution to be aided in all its work by an International Council.

It is earnestly hoped that this opportunity for establishing such a Bureau in the United States will not be missed, and that sufficient funds may be granted to at least set the project afoot, just now when its need, owing to the marked renewed activity in magnetic work, is most keenly felt. As it would be necessary to begin the work at once, temporary quarters for the bureau could be established in the Division of Terrestrial Magnetism of the Coast and Geodetic Survey, and much of the work of direction of the international work be done by the Chief of that Division, in connection with his regular duties.

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WASHINGTON, D. C., *January 27, 1902.*

[*Professor G. Neumayer, Director of German Naval Observatory, Hamburg, to Mr. Bauer.*]

[Translation.]

I take pleasure in acknowledging receipt of your esteemed favor of the 12th ult., enclosing a copy of the Plan for a Proposed International Magnetic Bureau of the Carnegie Institution, and to inform you that I have read the same with very great interest. I am of opinion that if this plan reaches its fulfilment, it is the most important step ever taken for the development of our knowledge of the earth's magnetism.

The thought which underlies this plan must appeal to every one who has ever been engaged in geomagnetic investigations. In no other branch of geophysics is it more essential to extend the inquiries over the entire earth. Magnetic research, to be successful, requires the cooperation of the most competent investigators of all countries.

As you know, I have occupied myself with the exhaustive collection of magnetic results and with their discussion, and it may therefore not be amiss for me to express my opinion regarding the possibility of success in this line of inquiry without the working together of investigators over the entire globe. Only by international cooperation, as is successfully done in the case of the geodetic and astronomical sciences, is it possible to prevent useless efforts and regrettable errors.

At the "Naturforscher Versammlung" in Hamburg last September (1901), I presented for Professor Schmidt, of Gotha, a plan agreed upon by us for the establishment of an institute for the discussion of geomagnetic results. To be sure, we had in contemplation only an institute for Germany. However, it was also the intention to include in its scope world-wide investigations. Your plan, however, as embraced in your proposition, is far more comprehensive and promises the greater success in case it should be carried out. Through correspondence with Professor Schmidt, I learn that your plan has his indorsement.

The salient points in your plan have been so well thought out, and have so thoroughly the impress of a truly international cooperation in terrestrial and cosmical magnetic investigations, inclusive of atmospheric electricity, that at present I am unable to add anything in the way of suggestion.

Permit me, therefore, in concluding, to express the hope that your plan may meet with success, so that at last we may reach the goal and be able to penetrate more successfully the mantle of darkness still enveloping the phenomena of the earth's magnetism and electricity, thus adding one more to the already notable scientific achievements of the American nation.

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I assure you that it will always be a pleasure to me to assist you to the best of my ability in carrying out the proposed plan.

HAMBURG, *February 11, 1902.*

[*Professor E. Mascart, Director of Bureau Central Météorologique, to Mr. Bauer.*]

[Translation.]

The project which you had the kindness to communicate to me in your letter of January 13 seems to me to be of very great scientific value. If it were possible to secure a participation in Mr. Carnegie's foundation, a first class piece of work could be created.

The profound knowledge of the distribution and variation of the earth's magnetism all over the globe would, besides its evident service to navigation, not fail to contribute to the progress of several other sciences, especially to that of geology, electricity of the atmosphere, and even to astronomy, on account of the still unknown influence of the changes of the surface of the sun.

The science of terrestrial magnetism is, by its nature, essentially international, for it can be treated effectively only by the cooperation of observers of all nations, stationed on land and sea.

The erection of an international bureau of the kind proposed, in the United States, would give to these investigations a mighty impulse.

The program you have worked out seems to be very well prepared in its general outlines.

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I shall speak about this matter before the Bureau of Longitudes, which occupied itself with this question some time ago, and I shall communicate to you the result of the discussion of this subject.

PARIS, *January 30, 1902.*

[*General L. Bassot, President of the Bureau des Longitudes, to Mr. Bauer.*]

[Translation.]

The Bureau of Longitudes has recently been informed by M. Mascart of the project for the organization of an International Magnetic Bureau of the Carnegie Institution. Our association would see with great satisfaction the realization of this project, which concerns terrestrial and solar physics in as high a degree as it does navigation.

The "Bureau des Longitudes," ever since it was founded, has always seconded, as far as its feeble resources allowed, all efforts which would tend to an increase of our knowledge of terrestrial magnetism.

* * * * * * *

I have the honor to send you the first pages of a general report, now in course of print, which will appear in the "Annals of the Bureau des Longitudes." You will see that the bureau has taken an initiative analogous to that which is proposed to the Carnegie Institution.

It was of the opinion, as you are, that in an enterprise of such proportions one isolated nation would be powerless to bring together, in a sufficiently short time, the elements for a magnetic chart of the globe; it has also made an appeal to the magnetic and meteorological observatories of all nations.

A perusal of this document will show you that the "Bureau des Longitudes de France" is especially prepared, by its methods, its

instructions, and the observations already collected, to second the realization of your project. It is gratified that its efforts have been appreciated and would be happy to see one of its members officially associated with the organization of this important international undertaking.

PARIS, *March 24, 1902.*

[*Professor A. Schuster, Director of Physical Laboratory, Owens College, Manchester, to the Carnegie Institution.*]

I have seen a proposed scheme for an international magnetic bureau, on which I should like to make the following observations:

I believe that no material progress of terrestrial magnetism is possible until our knowledge of the magnetic constants of the great ocean basins, especially the Pacific, have been determined more accurately than they are at present. There is reason to believe that these constants may be affected by considerable systematic errors. It is possible that these errors have crept in by paying too much attention to measurements made on islands and along the sea coast. What is wanted is more numerous and more accurate observations on the sea itself. I have had occasion recently to consider this matter very carefully, and I have come to the conclusion that the observations that are going to be made in the Arctic and Antarctic regions will be of very little use to us until we can supplement them by measurement in other portions of the ocean. It would be most useful, to my mind, to make a complete survey round the world of two circles of latitude, one in the northern and one in the southern hemisphere, say 50° N. and 40° S. of two circles of longitude, say 150° E and 100° W., taking them as far north and south as can be done without much trouble. As regards reduction of observations, there can also be no doubt that private enterprise is no longer capable of dealing with it. Anybody who has not a staff of computers at his disposal is at present incapable of working out any ideas he may have.

The problems which might be worked out are all of very considerable scientific importance. Whether they are also of practical importance is not possible to affirm, but such practical utility is by no means excluded. The other investigations mentioned in the scheme are also of importance.

MANCHESTER, *January 26, 1902.*

[*Professor W. von Bezold, Director of the Prussian Meteorological Institute, to Mr. Bauer.*]

[Translation.]

Your kind favor of the 13th ultimo [enclosing copy of plan of proposed International Magnetic Bureau] gave me very great pleasure. I have always had the feeling that it is comparatively easy to solicit funds for expeditions and similar undertakings designed to collect scientific material, whereas it is very difficult to obtain means or the necessary scientific aid for the discussion and utilization of the data collected.

The difficulties and dangers to be overcome in expeditions evoke energetic young investigators and easily arouse interest in wide circles, while the onerous discussion of the collected material requiring tireless application and more penetrating insight is not valued in an equal degree; and yet it is the critical discussion of the observations which furnish the actual results of the expedition and make the real contribution to science. This is especially true of magnetic investigations. For the establishment and maintenance of magnetic observatories, and especially for exploring expeditions, large means have been available. The reduction and discussion of observations, however, have been made only incompletely.

For limited regions and for rather restricted purposes—*e. g.*, in the case of magnetic surveys of countries—most gratifying contributions have been made, but for all problems embracing the entire earth, there are most keenly felt gaps in our knowledge. Thus, for example, the immense material gathered by the International Polar Expeditions of 1882–'83 has been utilized only to a very small degree, and so also in the case of the present international work, conducted in cooperation with the Antarctic Expeditions, whereby observations *en masse* will be piled up. It cannot be seen at present how the prompt utilization and publication of the results is going to be accomplished. To be sure, Professor Adolf Schmidt, of Gotha, is at present engaged, with the aid of grants from the Prussian Academy of Sciences, to cover some of the above mentioned gaps; however, the means at his disposal are altogether too inadequate for the accomplishment of anything very noteworthy. I should therefore hail with delight, as in the interest of science, if a part of the most generous gift of Mr. Carnegie could be devoted to further magnetic investigation.

*   *   *   *   *   *   *

Above all, however, does it seem to me to be important to submit to a critical and comprehensive discussion the immense pile of observational data. This is all the more necessary because in recent times the obtaining of accurate data, owing to the advent of the electric car lines, is getting more and more difficult. Then, first, shall we reap the real benefit of the time, labor, and cost spent in the accumulation of observations.

BERLIN, *January 26, 1902.*

[*Professors J. Elster and H. Geitel to the Carnegie Institution.*]

[Translation.]

Professor L. A. Bauer has submitted to us for our consideration the plan which he proposes for an International Magnetic Bureau of the Carnegie Institution.

With the earnest hope that this proposal may meet with your approval, we beg leave to suggest that it would be in full harmony with the proposed plan to combine with the organization of international magnetic work also the inauguration of observations pertaining to the electric condition of the earth and of the atmosphere, even though this at present may be possible only to a limited extent.

As the principal electric problems, we might name the determination of the strength of the earth's electric field and of the electric conductivity of the atmosphere (the so-called dissipation of electricity), and the investigation of earth currents and the aurora.

Since these matters have been investigated only within comparatively recent times, the methods of observation and of reduction and the theoretical utilization of the results are as yet very imperfect. Nevertheless, there is reason to hope that, even with the present means, relationships between the electric phenomena of the atmosphere and the earth's magnetic phenomena can be disclosed.

At comparatively small cost for instrumental means and without adding very much to the work of the observer it would be possible, in our opinion, to institute systematic measurements of the electric intensity of the earth's field and of the conductivity of the atmosphere at a few magnetic observatories as widely distributed as possible. A few years' results at these places would then show whether it would be desirable to increase the number of stations or expand the work in other directions.

WOLFENBÜTTEL, *January 26, 1902.*

ARCHEOLOGICAL INVESTIGATIONS IN GREECE AND ASIA MINOR

Report By T. D. Seymour.

CONTENTS.

Itinerary.

Bearing in mind your commission to inquire and report with regard to excavations near the Mediterranean sea, I spent the months of April and May of this year in Greece, among the islands of the Ægean archipelago, and in Asia Minor. On my way to Greece I visited the excavations at Pompeii, with which I was already fairly familiar. On my arrival in Greece I visited the ruins at Eleusis and heard lectures by Dörpfeld, the distinguished head of the Athenian branch of the German Archeological Institute, and by Wilhelm, the accomplished head of the Athenian station of the Austrian Archeological Institute, before the ruins on and about the Acropolis and near the harbors of Athens. I then made three tours

with Dörpfeld, who has lived in Greece since 1878 and during this period has done more than all other scholars together for the advancement of knowledge of classical architecture and Athenian topography. In Asia Minor I was with Professor Richardson, who has been for ten years director of the American School of Classical Studies at Athens.

Since coming to England I have spent more than a month in the libraries of the British Museum, reading reports of explorations and excavations in lands "near the Mediterranean." In the course of my journeys in southern and northern Europe, I have been brought into close relations with several archæologists of distinction, including some who have been engaged in excavations and other explorations, and I have used my best endeavors to secure from them, as from all other sources, the information which you desire.

On the first tour with Dr. Dörpfeld our party visited the excavations of the American School at Athens, both at Corinth and at the Argive Heræum, those of Dr. Schliemann at Tiryns and at Mycenæ, of the Greek Archeological Society at Epidaurus, of Vollgraff at Argos; then, passing to Arcadia, we saw the results of the work of the Germans and the French at Tegea and of the British School at Megalopolis; then, crossing Arcadia, we observed the excavations of the Greeks at Lycosura. Thence we went to Ithome and to Olympia, where we spent three days; thence to Leucas (which Dörpfeld holds to be the Homeric Ithaca), to the classical Ithaca, and to Delphi, where the excavations of the French were carefully inspected. On this first expedition Dr. Dörpfeld had a party of about forty, mostly philologists, but several specialists in archeology. The most noted of the party were professor Diels, of the University of Berlin, and Professor Förster, of Breslau. Dörpfeld lectured from two to five hours every day, expounding the ruins with great care, so arranging his lectures as to touch on almost every topic of ancient architecture, though the order of the discussion was determined by the geographical situation of the ruins, the prehistoric palace of Tiryns and the Argive Heræum being followed by the visit to the sanctuary of Asclepius at Epidaurus, and this by the palace, fortress, and tombs of Mycenæ, etc. The company comprised so many scholars of special attainments that informal discussions often brought out details additional to the lectures. Special trains were in readiness whenever it was desirable and were stopped whenever Dörpfeld wished to show a particular object or view. For the trip to Leucas, Ithaca, and Delphi a special steamer was char-

tered, which should take the exact route desired and should stop according to the will of the head of the party. This first trip occupied fourteen days.

The second tour covered twelve days, and was primarily for a visit to the islands of the Ægean, but provided also for visits to Poros, in Peloponnesus, and to Sunium, Rhamnus, and Marathon, on the Attic coast, which could not be reached so easily from Athens by land. The specially chartered steamer stopped at Eretria, on Eubœa, at least once, and in some cases twice; at Andros, Tenos, Myconos, Delos, Syra, Paros, Naxos, Thera, and Melos, and at five stations on the shore of Crete—Heraclion (for Cnossus), Gurnià, Palæocastro, Phæstus, and Agia Triada. As on the trip through Peloponnesus, Dr. Dörpfeld lectured regularly, sometimes on the boat in preparation for the visit, as well as in the presence of the ruins, and in addition we had on Crete the hospitality and expositions, at Cnossus, of Arthur Evans, the discoverer; at Gurnià, of Miss Boyd; at Palæocastro, of Mr. Bosanquet, of the British School at Athens, and at Phæstus and Agia Triada, of the Italian excavators Parabeni and Pernier, Halbherr, the chief excavator, being ill. This second party was constituted much like the first, but was rather larger, and comprised more dilettanti. The steamer could accommodate more persons than could find horses and mules for the ride across Arcadia, and the life was less strenuous than in Peloponnesus, although the physical exertions required in the visits to some of the islands were not slight.

The third expedition with Dr. Dörpfeld was to Troy, setting out shortly after the return from the visit to the islands of the Archipelago. To Dörpfeld is due all the scientific results of Schliemann's excavations on the site of Troy—indeed, Schliemann died just before the Homeric city was recognized—and here, as at Olympia, every stone was familiar to him. One morning he lectured more than four hours without interruption. At Troy we remained for three days, listening to lectures for at least ten hours, but having some time free for our own independent observations; and from Troy we made an excursion to the heights of Bunarbashi, which before Schliemann's excavations was generally accepted as the site of Homeric Troy. The party to Troy numbered twenty five, being necessarily smaller than either of the others for lack of accommodation. Some of us were quartered in Schliemann's old barracks, near the citadel, but the younger members of the party were obliged to sleep in a Turkish village nearly a mile distant.

The three trips with Dörpfeld were planned to include every important site on which excavations have been made, either on the mainland of Greece or on the Greek islands, with the exception of Attica, which had been visited previously. Architectural and general archaological importance prevailed over historical interest or poetical associations. Thus the party was not taken to Sparta, nor to Thermopylæ, or the Vale of Tempe, where only general impressions were to be gained from a brief stay. The call at Marathon was the only exception to this rule, and this lay directly on the route, in passing from Eretria along the Attic coast.

In Asia Minor, our little party of four, by dint of vigorous exertions, and favored by excellent weather, accomplished in two weeks more than I had thought possible in the limited time at our disposal. Here, too, we visited almost every important site of past excavations: the British and Austrian excavations at Ephesus, the German excavations at Pergamon, Priene, Magnesia on the Mæander, and Miletus, and the French excavations at Didymi, as well as the less important explorations at Sardis and at Magnesia on the Hermus. At Pergamon, Priene, and Miletus we were guests in the houses built for the use of the German excavators, and had the use of their plans and reports for the elucidation of the ruins. We visited also the extensive ruins of Hierapolis and Laodicea, a trifle more than 100 miles from Smyrna, and were also at Thyateira and Philadelpheia, though we had no opportunity there to search for and make examination of any ruins. Thus we traversed a considerable part of each of the four great plains of western Asia Minor, in addition to the Troad, and saw the sites of the Seven Churches of Asia, and passed along the coast in a small Turkish steamer—so near as to give us a distinct view of the country from the west. Reluctantly we abandoned the plan of visiting the scene of the American excavations of Assos; this place is not easily reached, and Professor Richardson was obliged to leave for America.

Thus in the two months of my stay in Greek lands I had under the most favorable circumstances such a comprehensive view of the archeological excavations which have been made in those lands within the last forty years as few before me have had in the same length of time.

The Field Assigned as Limited by Circumstances.

The broad general field which you suggested for my examination receives from circumstances important delimitations. France and

Spain, in general, may be left to explore the remains of antiquity within their own borders, and the feeling is general that Algiers and Tunis are also the proper field of France, which is officially doing much for their exploration, and doing this far better than a foreign expedition, because of the convenient cooperation of civil and military forces in a region which is somewhat unsettled. Danish archeologists desired three years ago to conduct excavations in the Cyrenaica, which was only superficially explored by the English—Smith and Porcher—forty years ago; but conditions were found to be unfavorable, having changed for the worse since 1865, when they were, to say the least, extremely difficult. I have heard of plans made by those who are interested in Roman archeology for excavations on the sites of Roman towns in Africa, but I have no exact knowledge of them.

Egypt.—In Egypt the work of exploration is being done very well, and with sufficient rapidity, chiefly by two expeditions—one from America, with funds supplied by Mrs. Hearst, of California, well directed by Dr. Reisner, who is assisted by Mr. Lythagœ, and the other that of the Egypt Exploration Fund, which receives about half its support from America. This Egypt Exploration Fund has had, for more than a score of years, Dr. Flinders Petrie as the chief director of its work, and about fourteen years ago it instituted a much needed Archeological Survey of Egypt, which has published twelve annual volumes by Newberry, Griffiths, and Davies; and half a dozen years ago the fund established a Græco-Roman branch which has published five volumes of high importance, and is said to have on hand a mass of fragments of papyri which weighs tons rather than pounds. Dr. Petrie in the course of the last twenty two years has done more than any one else ever did in the field for the excavation of Egyptian monuments. In addition to these two principal expeditions, the Germans are exploring in a limited way, with an annual appropriation of about $2,500; the French have recently dug a little with or through Arab explorers, but with comparatively unimportant results; and for the next five years the University of Chicago plans to have a small expedition in Egypt, under the direction of Professor Breasted. If Alexandria, a Greek city on Egyptian soil, is considered apart, as it well may be, we may note that a commission of the Society for the Promotion of Hellenic Studies, after a careful examination, reports that excavations there would be very expensive, and do not promise to be remunerative.

Egypt is not to prove an inexhaustible mine of antiquities, as

many seem to think. An experienced official of the Museum at Cairo believes that the great sites and fields of exploration in that land will be virtually exhausted within fifteen years, and that later explorers will have only a gleaning from the harvest. A more cautious scholar thinks that the opportunities for exploration in Egypt may continue for twenty years yet, except the supply of classical papyri, which is likely to be exhausted earlier; but the work of exploration is so well done now and is so largely under American influence that no reason exists for another special expedition to Egypt. A multiplication of agencies in so limited a field is not desirable, and no new leader would accomplish so much with the same means as the present explorers.

Turkey.—The Turkish government theoretically approves of archeological explorations in its territory, and allows them under certain conditions. Naturally, the consent of the owner of the land on which excavations are to be made must be secured, a government inspector must be paid, etc.; but these are mere matters of detail. Practically, however, the necessary firman for excavations is often delayed. Every request for such permission must receive the approval of the Sultan, and since the supreme head of the Turkish Empire allows little authority to his ministers of state and holds in his own hands all the reins of the chariot of government, so that many matters of importance to the realm are left without attention, no one can feel surprised that petitions which interest no Turk are often and long postponed. An American, Dr. Banks, recently has had action on a request for permission to conduct excavations delayed for more than two years (I think, for five years) on one pretext or another; one site proved not to be available in the opinion of the government, because the neighboring tribes of Arabs were unruly and might cause trouble; another site was not available because part of it was occupied by Turkish graves; and similar excuses are found without difficulty by Turkish officials. But in the past, English and Americans have been allowed to make excavations in the Turkish Empire—witness the American excavations at Assos and at Nippur, and the British excavations at Koyunjik and at Ephesus; and excavations are in progress at present in Asia Minor under the care of Germans, Austrians, and recently by the French. So, with patience, permission may be secured for any American excavations in that region. Well informed persons believe that Dr. Banks's request was opposed by an influence which would not be exerted so strongly against excavations near the Mediterranean. No longer would it be possible for a for-

eigner to dig as Layard did in Assyria in 1845 and Schliemann at Troy in 1870, and di Cesnola on Cyprus, also in 1870, without formal permission from the Porte. The official eye is watchful of excavations. The Turkish law no longer allows a general permission for archeological excavations; the firman is granted for only two years, and may be withdrawn if the excavation is not begun within three months; it is for a single definite place, no more than ten square kilometers in area; and the Minister of Public Instruction may order at any time the suspension of the excavations. Practically, however, when a firman of this kind has once been granted, it is renewed with little difficulty. Doubtless the Porte has been more ready to grant special privileges to the Germans because of the intimate relations at present of the two empires.

Syria.—In Syria two towns near Tyre are reported to show indications of relations between the Carthaginians and later Tyrians or of old Phœnician settlements. These might afford information greatly desired just now. The discovery of a wealthy and artistic civilization in Crete, of the second millennium B. C., which had close relations with Egypt and yet was not dominated by it, rouses special interest in the inquiry with regard to the relations between Crete and the western shore of the Mediterranean. Did the Philistines go to their later homes from Crete, as some have thought, or did they from Syria influence the island? The Rev. Dr. Eddy, who has spent his life near Sidon and has kept in touch with archeological work, recommends the towns referred to, and thinks also that $5,000 or $6,000 expended in excavations on the site of Dan would be very remunerative in results. His opinion is the more valuable since he is perhaps the chief adviser of the natives in their archeological finds.

A much more magnificent undertaking in Syria would be the exploration of the site of Antioch, on the Orontes, the most important of the cities founded by Seleucus Nicator, 300 B. C., in honor of the victory at Ipsus and named for his father, Antiochus—third in importance of the cities of the Roman Empire, being next to Rome and Alexandria, a city which played a great part in the history of christendom, where "the disciples were first called Christians," the patriarch of Antioch ranking with those of Rome and Constantinople, a city about which fierce battles raged during the Crusades. Antioch was the metropolis of the Orient, but it has been shaken frequently by earthquakes since its early ages. Ten times within the first six centuries it is said to have suffered greatly from this cause. The shocks of 457–458 and 526–528 A. D. are

said to have been particularly destructive. The Emperor Justinian rebuilt much of the town, but confined it in closer bounds. It is his walls that are preserved in ruins. They are said to be traced along a circuit of about 4 miles. In the fifteenth century the modern town had only 300 houses. Now there are said to be 5,000 to 10,000 inhabitants, but these occupy only a small part of the ancient enclosure. Thus excavations can be made there with slight expense for the surface of the ground. With Antioch should be included for exploration the neighboring Daphne, only 5 miles distant, where stood a great temple of Apollo and which was a favorite retreat of the Seleucids and the Romans. "For the architecture of but few cities of the world have we such connected reports for a period of 800 years," according to Professor Förster, of Breslau, who has made the most careful study of the ruins and who is most enthusiastic in his recommendation of this as a place for excavations. He would be glad to give to explorers any assistance within his power. His recommendation is seconded by Freiherr Hiller von Gärtringen, who has himself gained deserved note as an explorer and excavator. These excavations would cost not less than $50,000. They would throw light not only on the life of the Seleucid time, but also on the history of the Roman Empire.

Asia Minor.—In Asia Minor, in the course of the last quarter of a century, much has been done in the way of exploration, our country having an honorable part in this because of the work of Professor Sterrett, the results of whose labors fill the second and third volumes of the Papers of the American School at Athens. Professor Ramsay, of Aberdeen, the highest authority on the geography and history of Asia Minor, says in one of his books that he has had occasion to correct the work of many of his predecessors, but that when he has followed Professor Sterrett he has found but a small gleaning left to him after Sterrett's harvest. But the work of exploration in Asia Minor has been too sporadic to accomplish what is most needed, and often a single traveler has been over a route where at least three specialists were required. A party of three explorers—an epigraphist, an architect, and a topographer, with some knowledge of geology—who would supplement the work that has been done, would perform an important work for science. Even after the travels of Heberdey and Wilhelm, I am assured that very much remains to be done for the exploration of southeastern Asia Minor. Many ruins there deserve careful inspection and measurement, and possibly some might be found well worthy of excavation. Professor Sterrett is so

bound by his duties at Cornell University that he could not be expected to retain the conduct of such an expedition for two or three years, but he might be willing to take the leadership for a year, until his associates were thoroughly trained to the work. This work of exploration at present seems to me on the whole more important than the excavation of any known site in Asia Minor, although I shall go on to mention two attractive sites for excavation.

Excavations in Asia Minor have been chiefly of Hellenistic rather than Hellenic sites, and little of an early time has been brought to light. The American work at Assos, next to the great work at Troy, is the chief exception. On the other great sites of excavation little has been found of the best Greek period, to say nothing of the oldest Greek period of colonization. Apparently the earliest sites of some of these towns have not as yet been found, while Pergamon and Priene, of course, had all their glory in the later period. These explorations, then, have been disappointing in their results as regards the art and history of Greece at the time of its greatness, but they have rendered an important service in throwing unexpected light on the relations between late Greek and early Roman culture. Much that had been supposed to be of Roman development is now found to be of Greek origin in art, and particularly in architecture.

Of unexcavated sites in western Asia Minor, two seem easily preeminent—Hierapolis and Laodicea. These lie near together, about 100 miles from Smyrna, but on the Ottoman railway, so as to be easy of access. Both are superbly situated. Each is virtually deserted, Laodicea having no inhabitants, but bearing a modest crop of grain, and Hierapolis being haunted rather than occupied by a small band of gipsies. Each has remains of two theaters, baths, aqueducts, early Christian churches, and the like. It will be remembered that St. Paul in his letter (IV, 3) to the Church of Colossæ, which is only 9 miles from Laodicea, refers to the work of Epaphras also at Hierapolis and Laodicea, which proves that these were early homes of Christian churches. Hierapolis was founded about 200 B. C., on a high bluff which commands the plain between the Lycus and the Mæander and the great road from Sardis to the East—the road along which Xerxes led his forces in 481 B. C. and by which the younger Cyrus "went up" against his brother, according to the narrative of Xenophon, at the close of the same century. It was the birthplace of the philosopher Epictetus. The Apostle Philip and his daughter died there. Papias was bishop there in the second century of our era. It was the rival of Laodicea,

and connected politically with Pergamum, being "the center of native feeling, of Phrygian nationality in the valley." Inscriptions have been found there in large numbers; 363 are published by Humann. It was destroyed by an earthquake in the first century of our era, but rebuilt, as is supposed, by the help of the Roman emperors. The excavation of the entire site of Hierapolis is rendered impracticable by the thick deposits of limestone which have been left on the southern half of the ruins by springs which are heavily charged with minerals. These deposits since the destruction of the city are often 6 feet or more in thickness. Indeed, the bluff is a remarkable natural phenomenon. On either side of the ancient site is a petrified Niagara formed by these springs. The great theater, however, is well preserved, and lies so high that no limestone deposit has been made about it, and other important ruins also are free for excavation. The most thorough survey which Hierapolis has yet received was by a small party which remained only a fortnight, and hardly had time to turn over stones in order to see if these bore inscriptions.

Laodicea presents no such difficulties as the neighboring Hierapolis. The form of the long mounds which border the principal streets indicates that the rows of houses which lie beneath are concealed by no great depth of earth. This city was somewhat older than Hierapolis, being ascribed to Antiochus II and named for his wife Laodice. So far as appears, no archeological excavations have been made there; but the ruins were plundered somewhat for the construction of the Ottoman railway. An ancient Greek inscription from Laodicea lies face upward as a block in the platform of the neighboring railway station (Gondjeli).

Greece.—The Greek government allows responsible societies or institutions to conduct archeological excavations under government surveillance, recognizing the reasonableness of the desire to have the antiquities uncovered and not having money enough to undertake all of this work. The Greek governmental oversight is in no respect vexatious, but is designed only to secure a strict observance of the laws with regard to antiquities. Private persons are allowed to conduct excavations in Greece only in the name of some responsible institution; otherwise the government so limits the authority of the excavator as to make the work an official excavation at the expense of the individual. No man may even make archeological investigations on his own land without the permission

of the ephory, and the ephor will direct the work, determining even the number of laborers to be employed. Clearly, this law is intended to discourage irresponsible explorations, and it is entirely reasonable. Many foreigners would be glad now, as a century ago, to expend a little money in turning up the soil of Greece, although without any real scientific interest and not fitted, either by nature or training, for the conduct of scientific explorations.

Crete.—The Cretan law with regard to excavations is similar to that of Greece, but the government has granted permission to excavate to scholars whose position is assured. Mr. Arthur Evans's extraordinary skill is well known to the Cretan authorities, and after our countrywoman Miss Boyd's excavations at Kavusi in the name of the American School of Athens she could have had no difficulty in securing permission to dig on her own account, though I believe her present excavations are for the American Exploration Society of Philadelphia.

Italy.—The Italian government declines to permit foreigners to conduct archeological excavations on its soil, even in the way of mere superficial exploration. In the past such work has been allowed at times (as it was, freely, in the sixteenth to the eighteenth centuries), and hopes have been entertained that under suitable restrictions such permission might be again granted; but as yet these hopes have not been fulfilled, and the policy of the government seems fixed, although attention has been called to the inconsistency of refusing this privilege to foreigners, while it is granted to Italians who are believed to desire the permission not because of their interest in archeology, but simply for commercial reasons. But if, as has happened in recent years, a foreigner wishes to have some house removed which obstructs and covers important ruins, he can not conduct the work himself, but must give the money to the Italian government, merely expressing the desire that it should be used in a certain way.

Cyprus.—Since Cyprus came under the power of Great Britain, the ancient sites on that island are practically reserved for British archeologists. Small excavations have been made, it is true, by both French and Germans, but only by way of exception; in at least one of these cases the privilege was requested as a personal favor by an "exalted personage." Under ordinary circumstances the British Foreign Office would not grant to foreigners permission to dig for antiquities on that island.

Past Excavations.

Greece.—Thirty years ago no archeological excavations worthy of the name, according to present ideas, had been undertaken in Greece. The best had been the uncovering of the great theater of Dionysus at Athens, but this was not completed. The soil had been scratched a little on the Athenian Pnyx, a little rubbish had been removed from about the Parthenon, the French had spent six weeks in removing the earth from the temple of Zeus at Olympia, less time had been spent in clearing the sanctuary at Eleusis, many graves had been opened by unauthorized persons, and a few of the tombs in the Ceramicus had been opened by authority. Now, however, the principal and most promising sites have been subjected to a careful archeological search, and the list of places where important investigations of this kind have been made would read like a catalogue of the chief political, commercial, and religious centers of Hellas. Dodona, the earliest seat of the worship of Zeus in Greece, was discovered by Carapanos and excavated by him in 1875. Olympia, the seat of a famous oracle and of still more famous athletic contests—the chief common meeting ground of Greeks of all tribes, whether their homes were in Libya, Sicily, Macedonia, or Hellas proper—was excavated by the German Empire in 1875–'81 at a cost of about $200,000. Only in May of this year the French handed over to the Greeks the results of their excavations on the site of the sanctuary of the Pythian Apollo at Delphi, excavations which in their final stage lasted for ten years and which are said to have cost about what was expended for the similar work at Olympia. The site of the worship of Apollo at Delos was attempted by the French thirty years ago and the work was continued later, but was not completed according to modern standards, and it is to be resumed by them within a few months, an American, the Duc de Loubat, furnishing the money needed for this work. The ancient and honored shrine of Demeter at Eleusis, the seat of the Eleusinian mysteries, and the extensive sanctuary of Asclepius at Epidaurus, the chief seat of worship of the Greek god of healing, with temple, theater, stadium, and many other accompaniments of a health resort, has been excavated by the Greeks themselves rather gradually. The earliest and chief seat of the worship of Hera in Greece, the Argive Heræum, including not only the temples but also the neighboring porticoes and other buildings, was excavated by the American School of

Athens, assisted by the Archæological Institute of America, from 1889 to 1893. The temple of Apollo at Bassæ in Arcadia, from which the frieze was brought in 1812 to the British Museum, lay several miles from any town, and no indications have been found near it of any such complex of buildings as at Epidaurus or the Argive Heræum, but the ground immediately about the temple has been searched by the Greek Archæological Society. The French have investigated the site of the temple of Ptoan Apollo in Bœotia. The American School of Athens had the honor of uncovering the remains of what seems to have been the earliest site of the worship of Dionysus in central and southern Greece—Icaria in Attica, the home of Thespis, who was long considered the mythical founder of the drama, but who has assumed a clearer historical personality since these excavations. The ancient seats of wealth, culture, and power at Mycenæ and Tiryns were excavated by Dr. Schliemann. He dug also at Orchomenos, in Bœotia, where the work was resumed last spring by Professor Furtwängler, of Munich, who found interesting objects of a very early age, including specimens of the same early (non Greek, non Phœnician, non Egyptian, non Assyrian, non Cyprian) writing which covers thousands of tablets discovered in the palace at Cnossus, in Crete. The temple of Lycosura and its surroundings, which was called by ancient tradition the oldest town of Peloponnesus, has been excavated by the Greeks. The site of Corinth is in process of excavation by the American School.

At Athens more or less archeological digging has been done since it became the capital of the new Greek kingdom, from 1834 on. Gradually the summit of the Acropolis has been cleared of rubbish and from buildings of the Frankish and Turkish times, and the important structures on its slope—the Theater, the Odeum of Herodes Atticus, the Asclepieum and the Stoa of Eumenes—have been either brought to view, as the Theater and the Asclepieum, or have been cleared, as the Odeum. In the city of Athens systematic excavations have been difficult or impossible. When the seat of government was fixed there, seventy years ago, the site of the ancient city was covered by the Turkish town. Low, mean houses these were, and little money would have been needed for the purchase of a large district; but the Greeks then had *no* money to expend for archeological purposes. If modern Athens had been built at the Piræus, as some urged on other accounts, the solution of the archeological problem would have been easy; but the value of the land in the city has increased about as rapidly as the Greek government's means for

excavations. A peculiar and unfortunate "squatter's law," together with the carelessness of officials, allowed to certain "squatters" rights to land immediately adjoining the Acropolis, which was not occupied when the Greeks returned to Athens and which never should have been used for buildings. These have been dispossessed, but no thorough and systematic examination of the soil of Athens has been practicable.

The American School has conducted excavations at Eretria and Sicyon in addition to doing less important work on half a dozen other sites. The British School has excavated at Megalopolis, the French at Mantinea and Tegea. The Greek Archæological Society has conducted important excavations at Thermon, in Ætolia, the capital of the Ætolian League, and at dozens of other places, the income of the only lottery authorized in Greece being given to this society to be used for excavations and for the care of the ancient monuments. In the year 1902 the Greeks conducted excavations in twelve or thirteen places in their own kingdom, in addition to beginning the work of laying bare the ruins of the great temple of Hera, at Samos, which is still tributary to Turkey, though on a different footing from Chios and Lesbos.

Islands.—The islands of the Ægean sea have not been neglected either. The French have dug on Delos intermittently, and on Amorgos, The British have dug on Melos. The Austrians explored Samothrace. Freiherr Hiller von Gärtringen has excavated what may be called a Greek Pompeii on the island Thera. Early in the nineteenth century (1811) slight excavations were made on Ægina at the time when the sculptures of the temple of Aphæa were removed, and within the last two or three years Bavarian archeologists have carried these excavations further. For two years a Danish society has dug on the island of Rhodes. Other excavations also have been made on these islands, but it would be too long to enumerate all of them here. Even Belgians here have entered the lists of excavators. Englishmen have explored Kos, and one of their countrymen spent several weeks this summer on Karpathos, exploring it, as is believed, with a view to the discovery and excavation of Mycenæan sites.

Crete.—For many years archeologists have looked to Crete for the solution of many of their problems, and occasionally endeavored to explore it. About twelve years ago the Archæological Institute of America organized an expedition for Cretan exploration, but the conditions proved more unfavorable than was anticipated, and the plan was soon abandoned. Naturally, then, when this island was brought

under the protection of the Great Powers, and freed from the vexations and uncertainties of Turkish rule, not a few—among whom Italians, English, and Americans were most prominent—were eager to begin explorations and excavations there. Several Italians had made themselves familiar with the island, and have dug at Gortyna, Phæstus, and Agia Triada, near the southern coast. At Gortyna, a town mentioned by Homer as "well walled," was found eighteen years ago the longest law code which has come down to us from ancient times—from about the middle of the fifth century B. C.—of particular interest because of the recognized high reputation in antiquity of the Cretan laws. This was one of the sites on which our Institution hoped to make excavations ten years ago; but archeological exploration is not easy at this point even now, because of a watercourse and mills which involve vested rights. At Phæstus the Italians have found the remains of a magnificent palace of the same period as that at Cnossus—not so extensive, but rather better preserved and of equally impressive proportions. Agia Triada lies only about 3 miles from Phæstus. The excavations there are not completed, but the ruins seem to be those of a nobleman's residence rather than of a palace, in the English sense. Twenty five years ago certain discoveries seemed to indicate the site of the old palace of Minos at Cnossus, 4 or 5 miles from Candia, on the northern side of the island. Dr. Schliemann at one time nearly completed arrangements for the purchase of the land and the conduct of excavations, but some new difficulty arose and the negotiations were broken off; but as soon as the new government was established at Crete, Mr. Arthur Evans—a distinguished son of a distinguished father and the keeper of the Ashmolean Museum—who had long been waiting for this opportunity, with much patience and ingenuity in meeting difficulties and in overcoming them, purchased this land on his own account, and has there made the most brilliant archeological discoveries of the last twenty years. Since the final destruction of the palace, perhaps 300 years B. C., its site has been uninhabited and even untilled, so that in places the ruins were hidden by no more than a foot's depth of earth.

Miss Boyd began her excavations in Crete at Kavusi, on the northern shore, in the name of the American School of Athens, with part of the stipend received by her as fellow of that school. During the last two seasons she has dug at Gurnià, not very far from her former site, receiving her principal support from the "American Exploration Society," but using also her own limited means. She has found the

best example yet known of a "Mycenæan" village, with town chief's house, but no palace; and with a town shrine, the first such to be known of this period, though at Cnossus earlier and later shrines have been fonnd in the palace. At Palæocastro, near the eastern end of the island, Mr. Bosanquet and his associates have a Mycenæan town larger than Gurnià, but of no great splendor, though with very delicate and beautiful pottery. The cave of Dictæan Zeus has been explored by Mr. Hogarth. Less important excavations, like those at Zakro and Præsus, may be passed over here.

Opportunities in Greece.

Although excavations have already been made on the most noted sites and those which promised the surest and richest rewards for investigation, yet in Greece, too, interesting, important, and promising sites remain almost entirely unexplored. I would name Thebes, the Minoan promontory of Megara, Elis, Sparta (including Amyclæ), Gythion (the port of Sparta), and Samikon (near Olympia, on the west coast of Peloponnesus). I will write of these briefly in reverse order.

Samikon is thought by some to have been the western terminus of a "trade route" through Peloponnesus, from Gythion on the east, a thousand years or so before the beginning of our era. Excellent "polygonal" walls lie on it, and no excavations have been made there. Satisfactory trial excavations might be made for $1,000, in my opinion.

Gythion was not only the port for Sparta, but also the port for Amyclæ, the older capital of the Eurotas valley; and if ever a "trade route" crossed Peloponnesus, this must have been the great eastern terminus, whatever was the western. This site is urged for excavation not only by classical archeologists, but also by Mr. Flinders Petrie, who hopes that there will be found objects which will throw light on the relations between Greece and Egypt in early times. Little has been done in the way of excavations at Sparta, partly, no doubt, because of the improbability of finding great ruins. Thucydides says that in ages long after him men will hardly be ready to believe the former power of Sparta, while they will exaggerate that of Athens, judging from the ruins in each case. No one can hope to find a Parthenon or Erechtheum there, and no walls ever existed to be traced now, and no such mass of inscriptions as have been found at Athens was ever known in Sparta; but

many indications may be found of the early life, history, and institutions of the Spartans, and perhaps of their predecessors. Long ago C. O. Müller suggested that the real home of the Pelopids was to be sought at Amyclæ rather than at Mycenæ, and not a few indications point to this conclusion. Near Amyclæ have been found golden cups, ornamented with scenes of the bull hunt, in "Mycenæan" style, of an advanced type of art. Since the Cretan excavations and the discovery of the "Mycenæan" culture there, archeologists are eager to ascertain where this civilization was developed. On the discovery of the palace at Cnossus, many were disposed to regard this as the chief center of the "Mycenæan" art and life, but now some archeologists are disposed to turn their eyes back to Greece as the original home of this civilization, and since Argolis has been explored—at Mycenæ and Tiryns—the chief new light for this question, from Greece proper, must be expected from the region of Amyclæ.

Elis was the chief town of Western Peloponnesus, and almost nothing has been done as yet to explore its site. This name has come first to the lips of two or three archeologists when they were questioned as to the opportunities for excavation in Greece.

The very name of Minoa, at the harbor of Megara, reminds one of Minos and Crete and the Phœnicians. A recent ingenious writer counts this as the beginning of a Phœnician trade route through Bœotia, and, though we may not believe the Phœnicians to have had so strong a footing in Hellas as Bérard's theory implies, this Minoa is indicated as one of the landing points and trading ports of the Phœnicians, and thus as one where remains might be found which would throw light upon the early relations between the east and the west.

Thebes was the center of a large body of myths and poems. In the fourth century B. C., before its destruction by Alexander the Great, it had 30,000 or 40,000 inhabitants. After its restoration it may have had 10,000. At present it has only some 4,000 or 5,000 inhabitants, and occupies but a part of the old Cadmea, and fewer remains of antiquity are left above ground there even than at Sparta. The excavations which have been made there seem to afford perfect confirmation of the supposed myths as to the age and early influence of the city. Confirmation of the story of its connection with Phœnicia remains to be given. On the whole, I am disposed to recommend this site for excavation rather than any other in Greece, particularly since the railway from Athens to Thessaly, which has already

been completed into northern Attica, is to be extended at once to Thebes. This is sure to stimulate the rapid growth of the city as the center of a very fertile plain ; the price of land is sure to rise rapidly, and the experience of Athens is likely to be repeated there—houses will be built over the remains of ancient Thebes, and systematic archeological excavations will soon be put out of the question. A further consideration in this connection is that the digging of the navvies for the railway is sure to bring to light much material and many facts which would be of high value when combined with those secured by the archeological explorations.

I should like to write in detail with regard to the American excavations at Corinth. These are the most important archeological excavations in progress at present in Greece, and they have received marks of higher appreciation from archeologists abroad than from the public at home. Those who *complete* this work, naturally, will receive the credit of it, and others would be very glad to continue the explorations there, if the American School is obliged to abandon the site. European archeologists believe the results attained at Corinth to be large in comparison with the sum expended, which to the present time I suppose to be about $15,000, which is only a fifth of that expended at Ephesus, yet with as important scientific results. Excavation is not the chief work of the American School at Athens, and money for this work at Corinth has been secured with difficulty.

Opportunities in Crete.

In Crete, Miss Boyd's work certainly ought not to be allowed to end before she considers it completed. She has achieved important results with small means. She could not have done what she has if she had not paid a large part of her own expenses and found helpers who were ready to pay theirs. At Gurnià, last May, Dr. Dörpfeld spoke highly of her work, but after the party reached the steamer he called the company together and said that he should have spoken more highly of the excavations on the spot if he had not understood that it would be distasteful to Miss Boyd, who was present ; her work "could not have been better done." Her explorations at Gurnià are nearly completed. She would be glad, however, I am assured, to make a thorough exploration of the route which passes that town, from the northern to the southern shore of the island. This route is only about 10 miles in length, and seems to have been used in very ancient as well as in modern times ; it was used as a

"portage" by the French troops at the time of the recent military occupation of the island by the Great Powers. What support Miss Boyd is likely to receive from the society which has furnished her a small sum of money (I think only $2,000 this past year) for the work of 1901–'03, I do not know. Such explorations can be most economically administered with a larger sum of money to expend.

As for Miss Boyd's personality, I may say that she graduated at Smith College about 12 years ago, and is now instructor in archeology in that institution. In the year 1896–'97 she was a student in the American School at Athens, and was preparing to enter the competitive examination for a fellowship of the school. On the outbreak of the war with Turkey, however, she went with several Athenian ladies as a volunteer nurse to the hospitals of Thessaly, where she attracted much attention because of her unusual "capacity." In a later year she entered the fellowship competition which she had abandoned in the spring of 1897, and was successful. At the expiration of the year of her first fellowship she was appointed Hoppin fellow of the school, with a stipend of $1,000, half of which she used for her first excavations at Kavusi. Her life in Thessaly aided to give her an excellent command of the Modern Greek language, and brought her into touch with the Greek people in a way which has been useful to her in Crete. I should add that her reports of her explorations are admirably clear and methodical.

The suggestion has been made that America might join with Mr. Evans in his work at Cnossus, but he has nearly accomplished his great task. The great palace has been uncovered. It is true that an earlier palace lies in ruins under those of the later palace, and he will run some tunnels next year to learn what he can of the earlier without disturbing the later structures, but his work is substantially completed.

As for fresh sites of excavation in Crete, almost nothing has been done for the exploration of the western end of the island, which we may suppose to have stood in the closest relations to the peoples on the mainland of Greece; but no site there seems to be preeminently attractive, and a careful archeological survey and exploration of the island seems wise before further excavations are made, though such an exploration might discover almost at once a particularly attractive site. A French survey of the island under Ardaillon was planned a year ago, but I have not learned that it has been actually undertaken.

LAWS AS TO EXPORTS OF ANTIQUITIES.

The time is past when private or public museums can be enriched with works of art and curiosities by excavations. These now have to be conducted on a more ideal basis, for the advantage of the science of archeology and not as a commercial speculation on the part of the digger. In retired districts of classical lands some of the inhabitants have had much experience in finding and opening tombs for the sake of the treasures or trifles which were buried with the dead, and think this occupation to be more remunerative than agriculture; but all exportation of antiquities from the lands of ancient culture is now contrary to law. At the opening of the nineteenth century, when Greece was still in the hands of the Turks, Lord Elgin, by gifts and diplomatic arts, secured permission to "draw, model, remove, and excavate" any of the old buildings at Athens, and made large use of the right to *remove*, taking all that he wanted of the sculptures of the Parthenon, the frieze of the temple of Unwinged Victory, one of the Caryatids, a column and a long piece of the frieze of the Erectheum, the statue of Dionysus from the choragic monument of Thrasybulus, etc. Not much later, in 1811, two young Englishmen and two Germans stole the sculptures from the temple of Aphæa on Ægina, which are now the chief ornament of the Glyptothek, in Munich, and in the next year the same party, with one or two more, by a heavy bribe persuaded the Vizier of Peloponnesus to allow them to remove the sculptures from the temple at Bassæ, which were sold to the British Museum for £60,000. Mr. Hamilton Lang conducted excavations for antiquities on Cyprus in 1870 during two months without a firman from the government, and the sculptures are in the British Museum. About the same time General di Cesnola opened hundreds of tombs before he was obliged to seek the permission of the government; then he obtained a firman for the excavations which furnished the treasures for the Metropolitan Museum. Probably under the influence of the Cyprian excavations, attention having been called to the value of antiquities, Dr. Schliemann, in 1871, was obliged to promise to give to the Turks a share of the objects found by him at Troy, and difficulty was made in renewing the firmans. He conveyed away the most splendid of the gold treasures found at Troy, but later gave to the Turkish government $10,000 for its share of this treasure. From 1863 to 1874 Mr. J. T. Wood, an English architect, was engaged in excavations at Ephesus. He was allowed in 1863 to export all antiquities which

he might find, leaving the *duplicates* for the Turkish government; but in 1872 he reports that the Turks were disposed to grant no more firmans for excavations; they would do this work themselves. The Germans, however, were allowed to excavate on the site of Pergamon, beginning in 1878, on condition of giving to the Turkish government one third of the objects discovered, which third they later were allowed to purchase. The Americans were allowed to excavate the site of Assos (1879–1881), giving to the Turks one half of the objects found; and the Austrians were allowed to remove the sculptures from the Heroön of Gjölbashi, in Lycia (1881–1883); but these seem to have been the last firmans for excavation in Asia Minor granted by the Turkish government that include the right of exporting the objects found, or any considerable number of them. Excavations in the interior of Asia seem to be on a slightly different footing.

One of the first acts of the newly established Kingdom of Greece in 1834 was to forbid the exportation of antiquities. The Greeks had been humiliated and exasperated by the removal of the "Elgin marbles," which was followed soon by the abstraction of the sculptures from Ægina and Bassæ, and their government saw clearly that it would be mischievous to allow to pass freely from their country the memorials of their country's nobler past, and that not only professional scholars, but also other visitors, might be drawn to Greece for the sake of seeing its antiquities. The law was far in advance of public sentiment, however, and its influence has not been altogether beneficial. Men of learning and high position, professors in the University of Athens, not only connived at such smuggling, but were believed themselves to be dealers in antiquities to be delivered to the purchaser outside of Greece. I have myself known a man of distinction to amuse himself with outwitting the Greek custom house officials and conveying antiquities out of the country. Discoveries of antiquities, instead of being announced at once, were generally concealed, that the finds might be the more easily carried or sent from Greece. Thus the circumstances of the discovery, often of greater scientific interest than the object in itself, were concealed or passed unnoticed. Not infrequently also objects too large to transport easily in secret, such as statues or stelæ, were broken, and the large number of heads of terra cotta figurines without bodies offered for sale has been explained by the disposition of the finder to save only what he could most easily keep for himself. The mass of antiquities, large objects as well as small, which have reached the

museums of Europe and America from Greece in the course of the last seventy years is sufficient evidence that the Greek law against exportation is often evaded, and the efforts to elevate the public sentiment on the subject have not been very successful. The peasants in general do not yet understand why their government or any one else should care for broken stones or old pottery, and if strangers are disposed to pay a good sum for these trifles, why should they not have them? The Greek senate has recently passed a much stricter law, making every work of antiquity, wherever or however found, in Greece or in Greek waters, the property of the state, if this cares to take it for its museum. In this case the finder is to receive from the Archæological Society half of the value of the article. Every official of the government is bound under heavy penalties to see this law enforced. For dereliction of duty he is liable not only to be deprived of his office, but also to be sentenced to fine and imprisonment for two years. But very recently a party of archeologists visiting Eretria brought away with them vases that, in my opinion, were not only more numerous, but also more valuable, than those in the Eretrian Museum, all purchased within a few feet and almost under the very eye of the soldier on duty as a policeman. Though it is only fair to add that the best vases from Eretria were already in the museum at Athens, yet the letter of the law was flagrantly violated in the sale to the archeologists.

Contrast with Former Spirit.

The Turkish government in 1884 enacted a similar law to that of Greece, absolutely forbidding the exportation of antiquities; but this law, too, is not supported by public sentiment, and the exportation continues, though attended by difficulties. The European or American purchaser is obliged to pay a higher price because of the increased risk to the seller, and the high price in turn encourages illegal digging, and thus leads to archeological loss. The Germans, on renewing their excavations at Pergamon, receive for themselves only the objects that are needed to complete those which they removed to Berlin a score of years ago.

The great Museum of Cairo is already provided with a large supply of the ordinary objects found in Egyptian tombs, but the law forbidding the export of antiquities, except such as are derived from authorized archeological excavations, is so strictly enforced that a prominent dealer in antiquities has recently given up his business, it

being no longer profitable. A movement has been set on foot lately to forbid the export of any antiquities from Egypt, but this law is thought unnecessary, since the present statute virtually allows the Museum at Cairo to select what it wants from the objects found by the explorers.

One of the first acts of the new Cretan government was to enact a law similar to that of Greece, but still more stringent in its provisions. No antiquities may be exported from the island except those that are registered as such, with many formalities, on their exportation. The sentiment of the people on the subject is said to be good. Foreign excavators there pledge their word of honor that they will not convey from the island any of the objects which they find.

In all lands, however, the right of first publication is reserved for the excavator or finder.

Thus all future excavations in classical lands are to be conducted on the ideal basis—not for spoils, but for science. Twenty five years ago the Germans were called sentimentalists for undertaking the excavations at Olympia for the Greeks with the agreement that they should receive for themselves only duplicates and the right to make casts and photographs, and it was thought that they would have but few successors. Ten years later, however, the French proposed the excavations at Delphi without asking even for duplicates, if there should be any such. And the strictness of the Greek law has not checked the desire of foreign nations to take part in this work of uncovering the monuments of the past.

The motive in archeological excavations today is in marked contrast to that which prevailed in former times. Men have always been aware that objects of more or less value were often or generally buried with the dead, frequently, no doubt, in the belief that the spirit of the dead would be able to use or enjoy them in some way, and again often merely as a tribute of affection, just as flowers are laid upon a grave today. Men have been tempted from time immemorial to open tombs and to take whatever they could find there for their own use, though the act and the occupation of a grave robber were despised. Thus in some districts the majority of ancient tombs were opened and plundered in early times—many centuries ago. Of thousands of sarcophagi in Lycia, not one has been discovered in modern times which had not been opened previously. In other lands less of this work had been done in antiquity. A few years ago, according to report, three thousand Bœotian tombs which had not been robbed previously were opened by the country people before

the interference of the government, in the search for terra cotta figurines, which already were bringing a high price in the market, though not so high as at present.

In the fourteenth, fifteenth, and sixteenth centuries of our era, with the new interest in ancient literature was awakened also interest in the monuments of antiquity, particularly in works of ancient sculpture. Much digging was done in the neighborhood of Rome, on the sites of ancient villas, and many statues were brought to light. These were valued chiefly for themselves, however, as works of art, and if (as almost always) they were broken they were "restored" according to the ability, taste, and caprice of their owners. No one shrank from adapting the head of one statue to the body of another, if these could be made to fit by changing one or both of the parts, nor would the owner hesitate by an alteration of attribute to make a muse into an Artemis or a Demeter. This method of dealing with objects of antiquity continued until recent times. They were considered more valuable when "perfect," though the perfection were the result of a "restoration." Early in the nineteenth century the statues from the pediments of the temple of Aphæa on Ægina, having been purchased by the crown prince of Bavaria, were entrusted to the sculptor Thorwaldsen, who "restored" them most carefully and conscientiously, but archeologists now regret that they were "restored" at all. Canova deserves and receives credit for declining to add arms and noses and heads to the "Elgin marbles" from the Parthenon at Athens, when this was suggested. But only a third of a century ago most men of culture were not shocked—only a few archeologists were troubled—at the thought of a "restoration" of the statues found on Cyprus. The proper treatment of broken works of art, I may say parenthetically, has never before been exemplified so well as in the French treatment of the sculptures found in the recent excavations at Delphi, shown in the museum opened there last May: the broken marbles are set up as nearly as possible in their proper positions and relations, with no additions, while near them are placed plaster models with the missing parts restored according to the judgment of the archeologists and artists in charge.

The early expeditions for archeological purposes were insufficiently equipped, and thus desire for objects to fill museums appears as their exclusive aim. Even only thirty or forty years ago the "science of the spade" was in its infancy. Fr. Lenormant published in 1862 the results of his *Recherches archéologiques* at Eleusis two years before; but he has nothing to say except about the inscrip-

tions which he found there. Wood conducted excavations at Ephesus for more than ten years, but not as an archeologist. His aim was to find the great Temple of Artemis, and he cared little for the instruction afforded by the smaller objects which might be found in the course of the excavation. Few of these smaller objects came into his hands. They seem to have been considered a sort of perquisite of the workmen. He had not learned that a small object may be quite as instructive, though not so imposing, as a large object. During a large part of his excavations Wood was busy in his vocation as architect at Smyrna, and his explorations at Ephesus were left entirely in charge of his foreman, who laid not even the slightest claim to archeological knowledge, but was chosen simply for his skill in keeping the laborers at their work. Similarly Schliemann undertook his excavations on the hill of Hissarlik with no archeological preparation or associate. He dug at first simply to prove the truth of his theory that the hill of Hissarlik was the site of Homeric Troy. He put his first great trench 40 feet deep and broad in proportion through the upper part of the hill, exactly as a railway contractor would make similar cutting for his tracks. He had at work 150 men with barrows and carts, but not a single archeologist to watch or to advise. Of course some irreparable damage was done by the destruction of ruins in the upper strata with no adequate observation and record of them. After finding the great treasure of gold at Troy in 1873, and, still more, after discovering the wealth of gold array in the tombs of Mycenæ, Schliemann dug both as a treasure seeker and to prove the truth of his theories, but hardly as an archeologist. He seems to have been disappointed at not finding gold at Tiryns, and to have been disposed to consider his excavations there a failure until scholars of all lands declared his architectural discoveries to be more valuable than a mass of gold. At Mycenæ his interest in the treasures of the tombs was so overpowering that he took small pains to preserve the tombs themselves and allowed a considerable part of the "sacred circle" to be destroyed. But he learned to excavate by excavating, and attained considerable archeological knowledge and skill. To him more than to all other persons is due the extraordinary interest in modern archeological excavations, but his work had no scientific character, in the modern sense, until 1882, when he secured the cooperation of Dr. Dörpfeld. General di Cesnola, too, in digging in Cyprus, had had no more archeological training than Schliemann, and a large part of his work (according to current report) was done for him, like the excavations

of Wood at Ephesus. These men achieved important results, but in considering their work we must bear in mind that they were not archeologists at the beginning; they learned their trade by practicing it—by many costly experiments. Now all this method is changed. In the last excavations on the hill of Hissarlik Dörpfeld employed only one seventh as many common workmen as Schliemann had set at the big trench, and had with him three or four trained archeologists to observe, direct, and study.

In Egyptian explorations pains are taken to allow no scarab to be lost. Of slight "museum value" in itself, any such may supply the clue for the solution of some problem. In the excavations of Flinders Petrie in Egypt last winter, one object considered worth all the rest was a little image of the old king Khufu (Cheops), no longer than a man's hand, which would have escaped notice in excavations like Wood's. Modern archeological excavations are much more expensive than those conducted on the railway contractor's plan, but they are also much more instructive than those that were intended primarily to fill museums. Reinach, one of the most distinguished of French archeologists, in going a few years ago to dig at Myrina, in Asia Minor, where an indefinite number of terra cotta figurines had been found, said expressly that the expedition had as its first object not the finding of figurines, but to learn what was possible of the burials and the ancient life at Myrina. Now that the temptation to dig simply for objects of antiquity is removed by the law which forbids their exportation, archeologists are free to explore simply for the sake of science. Mrs. Hearst, of California, be it said to her praise, has given this direction to the party under Dr. Reisner which is digging at her expense in the soil of Egypt; they are to do what is best for the science of Egyptology, without regard to showy discoveries.

In another respect, too, the archeological excavations of the present differ from those of the past. Fifty years ago no one planned to lay bare the entire site of a sanctuary, and still less that of a town. Even at Pompeii until 1861 the excavators chose what seemed to be a promissing place here, and another there, as if they were picking blackberries in a mountain pasture. There, too, naturally enough, more attention was paid to the objects found than to what might be learned from the position of the objects, and after the antiquities had been removed to the Naples Museum no sufficient care was taken of what had been uncovered, but left, at Pompeii. Naturally, too, Wood's excavations at Ephesus were entirely un-

systematic. In order to find the great temple there he sunk trial pits for six years before he identified the sacred precincts, and dug for three or four months longer before he found the temple itself. More systematic digging would have found the temple sooner. And when he had found the temple, deeply embedded in a marsh, he dumped a considerable part of the earth which he removed from the temple upon the spot where we may believe the great altar to have stood. So the British Museum plans this year to resume those old excavations and to complete them according to modern methods. Similarly, at Delos, the French excavated buildings here and there, leaving between them in many cases spaces of unexcavated ground, with the result that the visitor has no connected view of the sacred precinct of Apollo as a whole. Here, too, because of the lack of a systematic and generous plan at the first, much of the earth has been moved twice, and part of it, I am told, three times. That the French expect to resume these excavations this year I have already stated. The early parties for the conduct of excavations and explorations in many cases were not well equipped with specialists, though these generally were not entirely lacking, as they were in the cases of Schliemann, di Cesnola, and Wood. The French seem to have been the first to send out companies for the purpose of investigating not only the "antiquities" and monuments of the country, but also its topography. The American excavations at Argive Haræum had a trained architect, but such a specialist is needed urgently also for the American excavations at Corinth, and has been needed in a less degree in the minor excavations as at Eretria and Sicyon.

Present Excavations in Greece.

At present excavations on Greek lands are carried on as follows, in addition to several minor excavations of the Greeks themselves:

Corinth: The American School at Athens.
Argos: Mr. Vollgraff, supported by a Hollander.
Tegea: To be resumed by the French.
Pergamon: The German Institute, under Dr. Dörpfeld.
Ephesus: (The Temple of Artemis.) Resumed by the British Museum.
Ephesus: (The City.) The Austrian Institute.
Miletus: The Berlin Museum, under Dr. Schrader.

Crete:

Cnossus: Mr. Arthur Evans.

Palæocastro: The British Society for the Promotion of Hellenic Studies.

Gurnià: Miss Boyd.

Agia Triada: The Italians, under Dr. Halbherr.

Samos: The Greek Archæological Society.

Rhodes: The Danes.

Delos: To be resumed by the French.

Leucas: Dr. Dörpfeld, supported by a Hollander.

Summary of Advice.

To sum up what has been said with regard to the opportunities for archeological research, one cannot easily, briefly, and safely compare the advantages of different sites. Between general exploration and excavation the balance might be turned by the possibility of forming a better party for one or the other sort of work. Corinth I regard as exceedingly important. Miss Boyd has done and is doing admirable work at Gurnià. The archeological exploration of the western end of Crete and of Asia Minor would be of real scientific value. The sites of Antioch on the Orontes, Laodicea, and Bœotian Thebes seem to me on the whole to be the most promising for a great excavation. Neither of the first two of these should be undertaken without the expectation of spending at least $50,000. Valuable work could be done at Thebes for less, although the sum named (to be expended in five years) would not be too great for thorough explorations there. A much smaller sum, perhaps $5,000, would suffice (so far as we can judge) for the exploration of the Minoan promontory of Megara and for trial excavations at Gythium, the port of Sparta. An exploring expedition to Asia Minor or to Crete should be dispatched, not for a single season, but for at least two. Three seasons would be still better.

Classical Archeology Worthy of Support.

Perhaps I may be allowed to add a few words with regard to the appropriateness of the work of archeological exploration in classical lands under the care and with the help of the Carnegie Institution.

(1) Classical archeology is now a science, and one in which many young scholars of our country are interested. For the most success-

ful study of this science, a direct and intimate acquaintance with the objects of antiquity is necessary. Laboratory work is as important in the pursuit of this science as for either chemistry or physics. The best practice is secured by fresh material, which rouses the student who is engaged in research to the fullest use of his powers, and this new material is secured best by excavations, the privilege of first study and publication being always reserved for the excavator. At the present stage of the science such material is peculiarly important, and our scholars are handicapped, as compared with others, if they are not provided with it. The science of classical archeology is important in itself; it may stand alone; but as a subsidiary to the study of ancient history and philology it deserves special consideration because of its relation to general education. The ancient histories of a few years ago have little more actual value than the chemistries and geologies of the same time, and the advance in our knowledge of ancient history is due primarily and principally to the work of archeology. And from all other sources combined, ancient literature—biblical and classical—has received, I think, less light within the last third or half of a century than from archeology.

(2) Our relations to classical archeology are peculiarly close, since this is our source of information with regard to the earliest culture from which we can trace our own. America rightly feels the special obligation to learn what can be known with regard to our predecessors on the western continent, but our modern life has been influenced to no appreciable extent by the habits and deeds of the North American Indians. Our intellectual inheritance has come from the Greeks and Romans, to whom we stand in the same relations as do our English and German cousins. All the learning and devices of Egypt and the farther East have not affected us directly. The history of our alphabet may be taken as an illustration. The Phœnicians traded with all the peoples of the Mediterranean and the Black seas—very likely even with the early inhabitants of Britain—but not one of all these peoples except the Greek had the skill to adapt the Phœnician alphabet to western use. Thus also Egyptian mathematics and Chaldean astronomy were received by the West only after being digested and assimilated by the Greeks. Still less did Egyptian and Assyrian art influence directly our own sculpture and architecture. Sir Henry Maine exaggerated only slightly when he said, "Everything that is not a law of nature is in its origin Greek." Since we are the intellectual descendants of the Greeks and Romans, and claim this relationship, the study of their lives and works is as

fitting for us as for any other moderns, and we might as reasonably leave the science of astronomy or that of mathematics to the other nations of the world, sure that it would not be neglected though we did not pursue it, as to leave to the peoples of Europe the advanced study of classical archeology and philology. Our manner of life and our literature are so founded upon those of the ancients that we cannot properly understand the present without an appreciation of the past. Largely through the opportunities offered by the American School of Classical Studies at Athens and its sister school in Rome, about two hundred of the classical teachers of our country have been brought into direct relations with the antiquities of Greece and Italy. We have as yet, however, only four or five classical archeologists who in training and attainments are worthy to be classed with the European university teachers of archeology. Work in connection with an important excavation or an expedition for exploration would do much for the training of the men who are to be the leaders of the science in America and who, we hope, will advance the whole science of classical archeology.

LONDON, *September 1, 1903.*

MECHANICS OF THE HUMAN VOICE

REPORT BY E. W. SCRIPTURE.

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INTRODUCTION.

I have the honor to report on a grant of $1,600 for prosecuting researches on the voice. As stated in the original application, I had on hand a large amount of unstudied material accumulated by a new method. The method consisted essentially in registering the human voice by the latest gramophone apparatus, and then tracing off the vibrations in great enlargement by a machine which I constructed specially for the purpose. This method has the advantage over all other methods of studying the voice by preserving the original speech as a gramophone plate (or phonograph cylinder) as well as furnishing the curves.

METHODS EMPLOYED.

Two quite distinct kinds of activity were involved, namely, obtaining the curves and studying them; the organization therefore included a tracing station and a computing bureau.

At the tracing station the curves are obtained from a gramophone plate—selected or specially made to contain any desired vocal utterance—by tracing off the vibrations by a special machine. Such a machine is suspended by springs or placed on a cement floor in a rather long room (50 to 100 feet). The machine is run continuously by an electric motor. The curves are traced on long bands of smoked paper; the speech curve appears as a thin white line on a

black surface. On account of its delicacy, the machine requires daily inspection and care by a mechanic. The renewal of paper occurs once every 12 or 24 hours, according to the length of the strip of paper, a factor that depends on the length of the room. Each renewal requires work for 1½ to 2½ hours.

The strips of paper are varnished before they are removed from the machine. A narrow band containing the curve is then cut out and mounted in the form of plates on pasteboard. Each plate is covered with a sheet of celluloid and is delivered to the computing bureau.

The records referred to in these investigations had been traced off by an apparatus known as Machine A. This machine was allowed to run at Yale University till March 1, at which date it was dismounted and turned over to the university. Owing to the fact that it was the first of the machines constructed, it could be successfully used only under my constant personal supervision; owing to the many changes in its construction and to the wear of four years running (often day and night), it had become somewhat deteriorated. It was decided therefore to build two new machines on principles learned by previous experience. These are now finished. One, known as Machine D, has been at work at Yale University under care of the mechanic who built it, and has completed the tracing of a plate containing a record of Dr. S. Weir Mitchell's voice. The other, known as Machine C, was finished in Europe and is about to be set up in Berlin.

Still another machine, of a different kind, had been constructed by a grant from the Elizabeth Thompson Science Fund several years ago. It traces off curves from a French phonograph with celluloid cylinders. Forty special cylinders of French prose and verse were made for it in 1902. It ran constantly from October, 1902, to May, 1903, and furnished the first curves of French speech ever obtained. These curves can be used for solving the problems of French vowels, French melody, and French verse, just as the curves for English were used for the problem of English speech. I have with me the complete tracing of a record of *Le Roi d'Yvetot*. It awaits funds for assistance in measurement and time for study.

The work of the tracing station was carried on at Yale University until September 15.

At the computing bureau the first work consisted of measuring abscissas and ordinates of the curves on the plates and in computing results according to certain formulas and methods. The

methods of computation employed by other investigators were first tested and found to be not fully adequate; they were therefore somewhat altered and developed and the approved methods were then applied to the plates on hand.

A large amount of material had been collected during two years' work of the tracing machine. This could not be studied because the measurements and computations required so much time. It was decided to concentrate the effort at the start on this material. It was found quite impracticable to carry this out at New Haven, one reason being the difficulty of securing intelligent labor at small rates. I therefore located in Munich, hired work rooms, and obtained labor by advertisements. Any number of doctors of philosophy, university students, and retired army officers could be found at 10 to 15 cents an hour, graduates of the Realgymnasium at 7½ to 10 cents, scholars from the Realschulen at 7½ cents. The great number at disposal made it possible to select specially reliable ones. As many as fifteen persons were employed at one time. The control of these persons, the systematization of the work, and the checking of results were placed in the hands of a retired Prussian major, the Baron von Hagen, at 25 cents per hour.

The organization was developed with German minuteness. Each piece of work—for example, the analysis of a wave—was kept in a separate little book; each worker had his particular task, and the books were passed in order from one to another. Every item of work was signed by the worker; when mistakes were found the person at fault was discharged. The separate books—nearly 500 in number—were classified and inserted in larger holders, from which they could be assigned systematically for working up any problem. A card index showed the progress and the material on hand.

The analysis of a single wave of a vowel usually required a minimum of 5 hours' measurement and computation. The study of each hundred waves from various vowels thus required at least 500 hours. The measurement for the melody of a single four line stanza of verse required the time of one person for 6 hours a day for three weeks.

The work of this measuring and computing bureau was carried on at Munich until October 1, 1903.

Problems Attacked.

The great amount of material accumulated comprised records of continuous prose speech and verse in English and German; they are

the first of their kind ever obtained, other investigators having confined themselves to single vowels sung into the apparatus (Hermann, Pipping, Bevier) or to single spoken words (Pipping).

Since the entire impression that passes by the voice from speaker to hearer is contained in vibrations of the air at the mouth of the speaker, the curves of speech can be assumed to contain solutions for all the problems of vocal expression. Among these problems are the following:

1. To determine the essential characteristics whereby one part of speech is distinguished in general from another—for example, the vowel *a* from the vowel *e*; or, briefly, the acoustic nature of speech sounds, particularly of vowels. Do the vowels consist of tones from the vocal cavities that have fixed pitches (Helmholtz, Hermann) or may they be of any pitch, provided they have fixed relations of pitch among themselves (Lloyd)? Are the resonance tones of vowels related to the tone from the vocal cords as overtones to a fundamental (Helmholtz); or do they have no such relation (Hermann)? If the latter is true, what is the explanation of such an apparent physical impossibility (Rayleigh)? Are previous investigators right in looking for the essentials of a vowel in the resonance tones alone? Why is the prevailing view of the nature and action of the vocal cavities inadequate to explain the thoroughly established results?

2. To investigate the rise and fall of the voice during speech—briefly, the characteristics of the melody of speech. In song the melody is apparently of a very simple nature; in spite of its use for emotional expression, the melody of song cannot convey all the shades of feeling that are possible by the melody of speech. This melody differs for every condition of mind, for every person, for every dialect, for every language. What are its fundamental laws?

3. To investigate the characteristics of rhythm in prose speech. Both prose and verse have their elements arranged on more or less regular systems in respect to stress, duration, melody, and other elements. A complete study of these elements for verse would solve the problem of the nature of verse. The theories hitherto proposed were established on the basis of judgments by the ear alone, and they certainly all quite miss the essentials of the case. They are theories of verse as it appears in type rather than of verse as it comes from the mouth of the poet.

4. To establish the types of American vowels. There are not 10 or 12 typical vowels, as often supposed, but more nearly a hundred of them, as distinct and as indistinct from each other as the races

of men. Some approach to the truth has been reached by lexicography in respect to the long vowels, but its results are largely erroneous in respect to the short ones. Both phoneticians and lexicographers are misled by spelling, and, by a well known psychological prejudice, vowels that are physically quite different appear to the ear as the same, and *vice versa*. For example, most phoneticians distinguish two forms of the so called indefinite vowel. Sweet (Axford) has lately recorded seventeen forms. In my curves I find several hitherto unrecognized forms, physically quite distinct, for nearly every vowel.

5. To study the differences of speech among various dialects. The phonetic survey of France (Guilleron) consisted in recording by ear the pronunciation of certain typical words in each district of France. The results were published as a series of maps, each one containing in phonetic type the pronunciation of the word at each place. The ear loses most of the differences in pronunciation; the type loses still more; the results are vague. The proper method would be to take records by the phonograph or the gramophone (as recently done by the Vienna Academy of Sciences for certain Brazilian dialects), and then to trace the curves by my method for accurate study. I find that phonograph records could be made with indestructible metal matrices and cylinders cast and delivered at a smaller cost per word than the atlas of Guilleron. Instead of maps with phonetic type, the student would have a collection of the speaking records. Various disappearing and changing languages (American Indian, for example) might thus be collected and preserved. The records might then at any time be traced off and studied by the methods used in these researches.

6. To determine the differences due to the speaker or singer himself, and to investigate the influence of training on these differences.

7. To investigate the influence of the emotions on the voice.

These last two lines of investigation would lay the foundations for the psychology and physiology of vocal expression. It would lead to data concerning the laws of expression in vocal music and in oratory; also possibly to the use of vocal records for determination of mental conditions in health and disease.

Of the work on these and other problems only the results for the first three will be considered in this report.

RESULTS OBTAINED.

1. THE NATURE OF VOWELS.

According to Helmholtz a vowel consists of the reinforcement, by the vocal cavities, of overtones in the tone from the vocal cords, these reinforced tones lying within definite regions for each vowel. Thus, for the vowel *a* the vocal cavity is so adjusted that it reinforces a certain tone; this tone, however, as an overtone, must stand in respect to its pitch in one of the relations 1, 2, 3, etc., to the tone of the vocal cords; that is, its number of vibrations must be an even multiple of that of the cords, and cannot be, for example, $1\frac{1}{3}$ or $4\frac{7}{10}$ times as great. According to Hermann a vowel consists of the presence of a tone or tones of definite pitch, the pitch being quite independent of any tone or overtone from the vocal cords—that is, the vowel tone may have, for example, $1\frac{81}{100}$ or $5\frac{16}{17}$, as many vibrations, etc. The two theories agree in asserting the presence of tones of a definite pitch or range of pitch for a particular vowel; thus for *a* there must be a tone of one pitch in the vocal sound, for *o* a tone of another pitch, etc. The two theories disagree as to the relation of this tone to the cord tone.

Helmholtz based his theory on attempts to manufacture vowels by reed pipes and by forks, with results that were not satisfactory. Hermann based his on curves of vowels sung by his own voice into a phonograph and then traced off. The material in either case was extremely limited. The results now studied under this grant comprised several hundred vowels by different speakers; being curves of really *spoken* vowels, they required new methods of treatment and gave results quite different from the rather artificial vowels of song.

It is to be understood that this work consisted of collecting specimens of speech of clearly evident excellence on gramophone plates, of tracing off the vibrations on paper in great enlargement, and of analyzing the waves of the curves by means of microscope measurement and mathematical formulas. Each single vibration of a vowel, for example, could be analyzed to show the set of tones from the cords and the vocal cavities. Each result stands as a fixed datum with which any theory has to reckon.

The results soon showed that neither the theory of Helmholtz nor that of Hermann could be applied. No explanation or new theory could be found until a suggestion was received from another investigation, as follows:

The validity of a vowel theory can be tested by constructing an apparatus to produce vowels on its principles. The Helmholtz theory was tested by Helmholtz himself in his vowel apparatus. This consisted of a series of tuning forks with pitch frequencies in the relations of 1: 2: 3, etc. By adjusting the resonators different overtones could be reinforced. Helmholtz states that he could obtain only a good *o* and *u*. The same results can be obtained almost as well by simpler apparatus and in a way to refute the theory. *Any* well made tuning fork of any moderately low pitch will produce a good *u*. Empty bottles often give good *u* vowels through several octaves of pitch. These facts made it unnecessary to construct another apparatus on the Helmholtz theory. I therefore began on the Hermann theory, which at the start I believed to be correct. According to this theory the vocal cords emit a series of puffs of air, each of which acts like a blow on the air of the vocal cavities and arouses the tones of the cavities. A blow of the hand on the open mouth will thus arouse the cavity tones. If we conceive the blow to be repeated often enough —*e. g.*, 100 or 200 times a second—we should hear a vowel. To produce these puffs of air I first used a siren comprising a disc with holes rotated before a blast of air. I found that a resonator with hard walls (brass, wood, etc.) would respond only when the puffs came at a definite pitch, whereas resonators with soft walls (cotton soaked in water, gelatine, etc.) would respond to any pitch. I could thus produce good examples of *a*, *o*, and *u* with a siren tone of any pitch. I then tried a *vox humana* organ pipe with the same results. These vowels were produced at *any* pitch of tone, as required by the Hermann theory. I utterly failed, however, to produce *e*, *æ*, or *i*.

Certain puzzling phenomena in the speech curves seemed to indicate that the action of the vocal cords differed for different vowels. Such an unusual conclusion, though difficult to accept, might be justified by considering (1) that the innervation of different parts of the vocal cords may differ for different vowels, whereby the distribution of the load, and consequently the mode of vibration, may differ (it should be noted that the vocal cords, or, more properly, the vocal bands, do not vibrate merely along the thickened edges, but through the entire muscular mass); and (2) that the vocal cords as soft bodies may be influenced in the character of their vibrations by the size of the resonance cavities. The experiments in manufacturing vowels were now renewed with tones from rubber membranes.

It was found that a combination of soft walled resonators with a yielding source of tone was adequate to produce all the vowels.

These experiments led to two conclusions: first, that one element in the nature of a vowel is a modification of the action of the vocal cords themselves, and, second, that it is possible to manufacture a machine that will sing the vowels.

With the first conclusion at hand the study of the curves was renewed from a different point of view and new methods of analysis were devised. The curves were found to be explainable on this theory. We thus conclude that in speech the vocal cords do not execute their vibrations independently of the adjustment of the cavities above them, but that the form of vibration differs for different vowels. Thus, when the vowels *a*, *e*, *i*, etc., are spoken at the same pitch, the vocal cords vibrate so as to emit puffs of air of different force and consequently of different timbre. One element in the nature of a vowel therefore consists of a modification of the vibration of the vocal cords—a result that, as far as I am aware, has never been suggested before. The consequences for phonetics, laryngology, and vocal culture have not been deduced.

The element of soft resonators must be considered at this point. The vocal cavities have hitherto been treated like resonators of brass or glass and conclusions have been deduced on this basis. Lord Rayleigh, for example, states that for very high tones the Hermann view of the independence of the cavity tone from the cord tone may be correct, but that for tones within an octave or two of the cord tone the Helmholtz overtone theory is certainly valid—a statement that would hold good for cavities made of brass or other hard material, but that is contradicted by every vowel curve obtained. The explanation is simple: the flesh forming much of the walls of the vocal cavities is soft, and nearly all the rest is covered by a moist membrane. The phenomena of resonance are quite different. As stated before, a brass resonator responds only to a tone or tones of definite pitch, whereas a resonator with walls of water responds to *any* tone. A series of artificial heads made of metal with the proper adjustments of the cavities could not be made to speak the vowels, but a series made of gelatine or similar material should do so. A mathematical treatment of the laws of resonance for soft cavities has not yet been developed, although it would be of value.

The theory based on the experiments in making vowels and the conclusive evidence for the speech curves shows how we must proceed in order to make an efficient vowel machine. The materials I

have used—gelatine, water, etc.—have to be renewed every few days. If they can be replaced by durable materials a vocal organ can be built that will sing vowels at any pitch, giving true human tones instead of the bleating tones of the present *vox humana* register. An apparatus of this kind is now being constructed on funds from another source; if successful it can be added to the regular church organs and used to sing the vowels during chants.

On the basis of the principles just stated, namely, modification of the cord vibrations by the adjustment of the cavities and softness of the cavity walls, work on the curves has led to the following further conclusions.

The resonance tones of vowels have definite regions of pitch which have definite relations to one another. For the effect on the ear changes in the region of pitch may be compensated by changes in the relation and by the introduction of new tones. Both Helmholtz and Hermann are therefore correct in assuming in general definite regions of tone for a particular vowel, but the tones can vary greatly if only the necessary compensations are made to deceive the ear.

It is to be noted that in their essentials the apparent contradiction between the theories of Helmholtz and Hermann disappears. We can, like Hermann, assert that the cavity tones are the independent variables; but when we add that the cavities modify the mode of vibration of the vocal cords so as to bring out certain overtones from the cord tone, we reach a result essentially in agreement with the theory of Helmholtz.

A great difficulty in studying the speech curves lay in assigning any physical meaning to a single wave of the curve. The curves did not permit any interpretations according to the usual views of resonance of overtones; otherwise mathematical analysis of Fourier would have furnished immediate results, and harmonic analyzers, like that of Michelson, would have made it possible to spare a great amount of work. A mathematical treatment of vibrations on new principles was out of my power, but a physical synthesis was attempted. A machine was constructed to register the curves of two superimposed vibrating springs of different dampings acted upon by magnetic impulses of different degrees of suddenness. If it can be made to furnish curves like those obtained from speech, we shall have the following results: 1. The principles of vibration on which it acts can then be assumed to be the same as those of the voice. 2. An atlas of curves can be prepared in such a way that when a given speech curve is found to be like a specimen in the

atlas, its components will be given at once by the known components of the apparatus curve, thus avoiding a long and expensive analysis. 3. Curves of pure speech sounds can be directly engraved on gramophone plates without any intervention of the voice itself. This apparatus, made in Munich, after a former rough model, is nearly completed.

In studying the curves another phenomenon was observed. A spoken vowel changes its character from beginning to end; in the hundreds of vowels inspected no two waves of a vowel were ever exactly alike. Neighboring waves were similar and the change occurred more or less gradually, according to the nature of the vowel. Except in long vowels the ear cannot detect even the existence of such a change, and all phoneticians have considered the short vowels as constant things. The ear gets only an impression of the general effect and what the vowel really sounded like at beginning, middle, and end remains unknown. I find that the ear classifies as the same short vowels a number of types having in reality no acoustic resemblances. I also find that a suggestion from spelling or from another word will cause the ear to hear as different several vowels that are acoustically the same, and as the same several vowels that are acoustically different. The phonetic spellings in the dictionaries are certainly to a considerable degree erroneous in respect to the short vowels. The study of short vowels should be extended to include many hundreds of cases, and a machine should be constructed that will make it possible for the ear to hear each stage of the vowel separately. In respect to the long vowels a somewhat similar condition was found; for example, the usual statement that long vowels, such as *ee* in "see," are diphthongized does not always, or even generally, hold good in ordinary American conversation.

To elucidate the curves of vibration from the vocal cavities it was necessary to make some study of the cavities themselves—for example, as formed by the position of the tongue. One method of studying the position of the tongue is that of employing an artificial palate with a chalked surface; the chalk is removed where the tongue touches. This method was devised in 1887 by the American physician Kingsley, who registered about a dozen contacts. For French the study has been systematically carried out by many hundreds of registrations by Rousselot (Paris) and his pupils; for German a few results have been obtained; for English nothing has been done. I have accumulated and prepared for publication many hundreds of registrations of the American sounds in their varieties, using not

only the former palate of hard rubber, but also a specially thin one of aluminum. I have also lately devised a palate of paper soaked in cobalt chloride, which registers automatically the regions of contact by changing the color of its surface.

2. The Melody of Speech.

Under melody we understand the rise and fall in pitch of the tone from the vocal cords. The pitch of the cord tone at any instant can be obtained from the speech curve by measuring the length of the group of waves corresponding to one vibration of the cords. An extensive study of English melody is of the highest importance for oratory, for the history of the language, for the psychology of the emotions, and, according to the recent discovery of Professor Sievers (Leipzig), as a means of textual criticism. Only three experimental studies of English melody have yet been made, all in my previous investigations. In the present investigation part of a speech by Senator Depew was studied ; it furnished data concerning American melody in a speech without oratorical exaggeration. A record by another speaker furnished data for American speech with satirical expression. The melody of Rip Van Winkle's toast, by Joseph Jefferson (previously published), was recalculated by more accurate methods ; it furnished data for emotional expression. In this way the characteristics of American melody for oratorical speech were obtained. When compared with the previous studies of the melody of American sentences and of the Lord's Prayer, the differences from conversational and religious melody became apparent. The work on melody should be extended to more persons, to different subjects, and to different languages. Problems of the following kind would be answered :

(*a*) Has each piece of prose or verse a charcteristic melody of its own that appears in spite of the individual differences among the speakers ? If so, is it possible on this principle to pick out collaborations and insertions from a text ? Professor Sievers asserts this to be true, and is just issuing an edition of the Hebrew bible with text criticism on this basis. His judgments are entirely by the ear as he reads the passages himself. The fundamental principles, however, should be established by experimental records. I discussed the matter last spring with Professors Wundt and Sievers in Leipzig, and the Psychological Laboratory there has, as a result, undertaken of its own accord some researches on German melody. At the

University of Munich the interest in this work led to loan of apparatus from the Psychological Laboratory and to active assistance by Professor Lipps.

(*b*) How does the melody of any given piece or of conversation vary with the emotions, with the speaker, with the dialect, etc.? Have the differences in melody anything to do with differences in character among different persons and among different communities? For example, does the rising inflection at the end of each sentence indicate as a permanent trait for all Saxony the dubitative feeling that it indicates when used by a person in any other part of Germany?

Quite a number of records of melody have been completely studied, but the work should be considerably extended before conclusions are finally drawn.

3. The Rhythm of Speech.

The same record that furnishes the data concerning the nature of the sounds (see 1, above) and the melody (see 2, above) also furnishes measurements of their length, or duration, and approximately of their stress. These are the data for deductions concerning rhythm or meter, and consequently of the solution of such problems as:

(1) The nature of English verse. Is it mainly quantitative (*i. e.*, distinguished by long and short syllables), or melodic (*i. e.*, by rise and fall of the voice), or emphatic (*i. e.*, by greater or less stress)? These are the points under discussion at present. My results lead to the conclusion that such questions are entirely aside from the essentials of the case. Verse, I consider, is a form of mental expression with periodic recurrences of greater and less mental effort. The problem is, then, to find how this greater effort expresses itself. I find that it may occur not only by greater stress or by longer duration, but also by a change in pitch, by decreased stress, by shorter duration, by difficulty of enunciation, by pauses, by contrast of thought, by emotional content, or by any other element that is appropriate to increased expression. This psychological theory of the nature of verse renders it possible to gather the various forms into one system explainable on common principles. The usual division of English verse into feet and syllables has no poetical meaning; no poet writes that way.

For carrying this investigation into detail I have had two gramophone records of verse made by an American poet. The tracing off of one of these required over two months' running of the machine;

it is now finished. The work should be promptly extended to records made by the greatest living poets.

(2) The nature of emotional expression in oratory. Prose has its rhythm also. The most melodious orations have rhythms often resembling those of verse. The changes in these rhythms, the introduction of rhythmic discords, etc., are unconsciously used by orators to arouse the emotions. The three speeches studied furnished a quantity of data.

Continuance of the Work.

1. Publication of the results already obtained can be undertaken, but it is not recommended at present. The records studied are mounted as 40 plates for reproduction. Of these 18 are already in blocks, leaving 22 still to be made. The only satisfactory method thus far found for reproducing the plates is that of copper etching, as used by C. P. Wright, West Fourteenth street, New York city, at a cost of $15 to $20 per plate. The preparation of the text of 400 octavo pages to go with the plates will require about two months more of labor. Not one fifth, however, of the information on the plates has yet been extracted, and the work should, in my opinion, be completed, before publication in many of its details, by further study of the plates already on hand and by work on new plates already furnished from the tracing station.

2. Provision should be made for the study of results already on hand. The study of the plates known as the "Yale record" (specially made for me), the "Woman record," the "Mitchell record number 3" (specially made), and the "A K A record" (specially made); also of the "Yvetot record" (specially made, French verse) should be at once continued under my personal care. For this I need to employ the labor of 3 persons for 6 hours a day for 150 days. At 20 cents an hour this requires $540. The incidental expenses connected with cutting, pasting, photographing, and measuring the records can be estimated at $150. The total outlay proposed would therefore be $690.

3. Provisions should be made for continuing the tracing of speech curves.

Machine D (gramophone records) should be kept running for the next six months, tracing off a set of plates (American vowels, American verse, etc.). This machine gives larger and more detailed curves than those obtained by the other machines. As the

records are specially made, the material is of such appropriateness that it could be profitably published at once as an atlas with instructions for study. This would place a great amount of new data in the hands of psychologists and linguists for investigation and publication; or publication may be reserved until arrangements are made for studying the results.

Machine B (French curves) should also be kept running for six months. During this time the machine can trace a record of French prose, a record of French verse, and a record of French words, each one of which, with the exception of unstudied records already in my possession, will be the only accurate curves of French speech ever obtained, and will furnish, for the first time, accurate data concerning the French vowels, French melody, French rhythm, and French meter. The curves could profitably be published at once as an atlas of plates with instructions concerning methods of study. If publication were postponed, Professor Grandgent, of Harvard, would presumably assign topics for doctor theses and deliver results for publication with the curves. As these curves would contain material for 8 or 10 theses, and as the interest is widely spread, I recommend the prompt publication of the plates alone.

These machines can be handled only by myself and the mechanic who has had four years' experience in making and running them. This mechanic (C. S. Smith, 119 Davenport avenue, New Haven, Conn.) is willing to continue the work at $18 a week, or $450 for the next six months. The incidental expenses—paper, paste, electricity, repairs, etc.—can be estimated at $75, making a total of $525. These two machines will run in his charge in America. The results will be delivered to me as my property, subject to the wishes of the Carnegie Institution in respect to publication.

4. Further new gramophone records should be specially made for these investigations. My experience has led me to formulate the following principles:

(*a*) The persons selected for the records should be of such prominence that their biographies are and will be obtainable from cyclopedias, histories, and biographical dictionaries by any one studying the results. I often find it difficult to obtain the requisite data directly from persons of no distinction. All my efforts have been fruitless to obtain, directly or indirectly, any information from the still living speaker of the first record I studied. In any case, persons elsewhere may study such speech curves from other points of view and may wish different biographical data from the ones I have

collected. Still more important is the consideration that a decade or more hence entirely different and unforeseen problems will be considered of the most importance, and, although the data can be found in the curves, the personal data will be lacking.

(*b*) The metal matrices for all records should be preserved, in duplicate or triplicate, in fireproof buildings, and copies of the record should be obtainable by any one at reasonable cost. One of my most important studies has been of a private record by Joseph Jefferson. I possess only two copies of the record, one of them already much worn. No more copies can be obtained, and the matrix cannot be found. No other record of Jefferson's voice exists. Of another record all my copies but one have been worn out, and the matrix is known to have been destroyed.

These considerations suggest the following plans: For all future records specially made for me the American and German gramophone companies have offered to furnish—in strictest privacy and free of charge—duplicate or triplicate metal copies (one in indestructible material) of the matrix, provided the records shall be of such historical and scientific value as to justify the cost (a minimum of $100 for each record) and provided proper places of deposit shall be found. The company will consider these records as the property of the person speaking, of the place of deposit, or of myself, according as I may adjust the matter, and binds itself to make no use whatever of these records for its own purposes. I have already arranged with the National Museum, with the Library of Congress, and with Harvard University for depositing such matrices and records. In compiling a list of persons whose voices would, for one reason or another, presumably be of historical importance in the future, and thus worthy of the expense of registration and preservation, I have had the advice of President Gilman, President Eliot, Mr. John Hay, Professor Asaph Hall, Mr. Henry C. Lea, Mr. Joseph Jefferson, Mr. John La Farge, and others. These records will form the nucleus of a phonetic archive of the voices of distinguished or interesting persons. A similar archive is to be founded in Berlin, and I am now arranging for an interview with his Majesty the German Emperor. Copies of such European matrices and records as may be of interest to Americans will be delivered to me for deposit and study. In America I propose to collect the voices of prominent statesmen, business men, scientists, writers, divines, orators, singers, actors, and others. In Europe I shall begin with the voices of rulers. In the collection and investigation of such specially made

records great weight should be laid on those of poets and orators. In the first place, the printed verse or speech can convey only a part of the intention of the author; a good gramophone record by Patrick Henry or Longfellow would give something that is now forever lost. In the second place, from accurate speech curves traced from such plates we can obtain for the first time reliable data concerning the essential factors of oratorical speech and of verse.

The plans on both sides of the Atlantic are complete. I am ready to carry them out privately or under the direction of the Carnegie Institution, according as the Trustees may direct. In the former case the matrices and records will remain my personal property. There will be no expense except for traveling.

5. Provision should be made for the construction of the following apparatus:

One piece, suggested by Dr. Billings, is designed to turn the records of the speech curves back into sound. The difficulties with the design have been great, but have now been overcome. The success of a speech record depends on allowing time for the point of the reproducer to follow the fluctuations of the groove. The larger the disc or cylinder the better the result. The speech waves are thus very long and flat, and often cannot be distinguished by the eye from a straight line. In my tracing machine the amplitude of the fluctuation is greatly increased, while the length of a wave remains the same, or is even shortened. Thus the fluctuations become visible. Such waves cannot be used for producing sounds directly. They must be greatly drawn out in length and reduced in amplitude. For this purpose the speech curve is in the new apparatus to be cut on a wooden cylinder and reproduced as a depression in wax on a phonograph cylinder. The former cylinder is rotated at a very slow speed in comparison with that of the latter, while the amplitude is appropriately decreased. The minor technical difficulties are, of course, numerous, but the machine can presumably be finished before next spring.

The value of such a machine lies not only in the fact that it can be used to test the accuracy of a tracing by turning it back into sound (it can be more readily tested in other ways), but particuarly in the fact that it opens up an entirely new method of studying the voice. A vowel consists of a series of vibrations that are not of constant character. The form of the curve always (in my results for American English) changes from beginning to end, wave by wave. The sound must therefore be different at each instant.

Except in very long vowels, the ear cannot hear any change, but gets a general impression. This is necessarily so crude that trained phoneticians will assume utterly different vowels in such a word as "not," even when spoken on the same occasion by the same speaker. The changes within the vowel lie, of course, beyond the grasp of the ear. This new machine will furnish the means of an *acoustic analysis* of the vowels in the following manner: The first wave of the vowel, as it appears in the tracing, is engraved on the wooden cylinder in a continuous repetition. The reproduction on the wax cylinder then gives continuously the sound for the first wave of the vowel. This is done for each wave in succession. In this way the acoustic vowel elements are determined throughout a vowel. The cost of this apparatus can be placed at $300.

A special measuring machine should be constructed. At present the curves are enlarged by photography and measured directly in tenths of a millimeter. To avoid the enlargement, an apparatus was designed in which a magnifying glass can be moved by millimeter screws in two directions above the curve without touching it, the readings being in hundredths of a millimeter. The construction was not begun, as the estimated cost (mainly for the fine screws) was $200. It should be made at once, however, as the photography is a heavy running expense, which can be avoided once for all by the apparatus.

6. A mathematical treatment of the phenomena in resonators with soft walls and of the vibrations of soft bodies should be sought. In the works of Helmholtz and Rayleigh these problems are quite overlooked, and yet the human voice—whose vibrations are produced in this way—is the most important of musical instruments. Lord Rayleigh, in the second edition of his Theory of Sound, has shown great interest in the study of vowel sounds. I suggest that he be requested by the Carnegie Institution directly to treat the subject. I would take great pleasure in laying before him any desired curves of speech or results and in carrying out any experiments, measurements, or computations he may wish. If he is not willing to do the work I shall be pleased to cooperate with any one whom the Institution may suggest. Presumably, no cost will be involved unless Lord Rayleigh desires a personal interview in England. In this case traveling expenses will have to be covered.

FUNDAMENTAL PROBLEMS OF GEOLOGY

REPORT BY T. C. CHAMBERLIN.

I have the honor to submit the following report of progress on the work done under Grant No. 31, made for the promotion of investigation on certain fundamental questions in geology. The period covers nine months, from January 1 to October 1, 1903.

Immediately on the announcement of the grant an arrangement was made with the University of Chicago by which I was relieved of all educational work except that directly connected with investigation, and even this class of university service was limited to six months of the year. Arrangements were also effected for relieving me, so far as practicable, from executive work. A research assistant was provided, a feature of the understanding being that the University would support at least as much investigation in geological lines as it had done before. The University has continued to pay one half of my previous salary, and the other half has been paid from the grant.

Plans for collaboration on different phases of the complex subjects of investigation, of different extents and degrees, were soon arranged with Dr. F. R. Moulton, of the University of Chicago, on questions relative to the origin of the earth and related subjects, especially those involving celestial mechanics; with Professor L. M. Hoskins, of the Engineering Department of Stanford University, on questions relating to earth stresses and cognate subjects; with Professor C. S. Slichter, of the Mathematical Department of the University of Wisconsin, on questions connected with the rotational effects of tidal action; with Mr. A. C. Lunn, of the Mathematical Department of the University of Chicago, on questions relative to the generation and distribution of internal heat by compression and related subjects; and with Professor Julius Stieglitz, of the Department of Chemistry of the University of Chicago, relative to the bearing of certain ancient evaporation products on the former state of the atmosphere. Mr. W. H. Emmons, of the Department of Geology of the University of Chicago, has served as research assistant under appointment by the University of Chicago.

In pursuance of the plan of collaboration with Dr. Moulton, it was thought best at the outset to examine critically everything of any

moment that had been written on the origin of the solar system. This had not advanced far before the conviction arose that the preparation of an analytical review of the whole literature of the subject would save other investigators the very considerable labor of repeating the task undertaken by Dr. Moulton and would in other respects be a valuable contribution to investigation. In pursuance of this the literature, which is quite voluminous, has all been read with extensive annotations, and the preparation of the review is now in progress and will be submitted as soon as finished. Subsidiary to this, Dr. Moulton has aided in the informal discussion of other subjects under investigation.

As the origin and distribution of the earth's internal heat is fundamental to many of the most important problems of geology, a large part of our joint studies has been given to the continuation of previous studies bearing on this subject; but, as all fundamental questions react the one upon the other, we have endeavored by the scheme of collaboration adopted to advance coordinately several lines that seemed most promising of mutual helpfulness.

An analysis of the work heretofore done on the origin and distribution of internal heat and of its sequences reveals the fact that inquiry has largely been guided by the older and the least probable of the three hypotheses of the origin and initial distribution of heat that are now recognized. Under the long dominant view that the earth descended from a gaseous spheroid through the molten state to its present condition, there naturally arose the conception that during the liquid state convections stirred the molten mass from center to circumference and measurably equalized the temperature, so that the whole was cooled down equably until it approached the temperature of solidification and became too viscous for further effective convection. As the effects of internal pressure on the temperature of solidification were unknown in the earlier days, and largely remain so still, a *uniform* internal temperature, not far from the melting temperature at the surface, was postulated. The deductions of Lord Kelvin, which have exercised great influence on geological opinion for the past half century, are based on the assumption of a uniform initial temperature of 7,000° F. throughout the interior, and similar assumptions of practical uniformity, with unimportant numerical variations, have been made by other able investigators. Computations on this basis, with reasonable assumptions relative to the subsidiary factors, lead to the conclusion that effective cooling could scarcely exceed a depth of 200 miles in a period of 100,000,000

years, and not more than 350 or 400 miles in the enormous period of 600,000,000 years. The inference inevitably follows that all the movements and deformations of the crust of the earth, due to cooling, are confined to very shallow depths relative to the radius of the earth, and that the greater interior mass of the geoid has not appreciably participated in thermal effects. This conclusion, if true, is radical. Assumed to be true, it has profoundly influenced geological inquiries and interpretations.

With the growth of evidence that the temperature of solidification rises with pressure, there has grown the hypothesis that the earth solidified first at the center, because of the high pressure there resident, and later congealed at higher and higher horizons in succession until the solidification reached the surface. This view has come to replace the older view to a large extent, in competent opinion, but the necessary corollary that the initial temperature of the solidified globe would be high at the center and grade thence to the surface, though recognized, has not replaced in an equal degree, in dynamical studies, the older view of an essentially uniform thermal distribution. As a result, we have no system of dynamical doctrine worked out consistently on this later hypothesis.

In attempting some years ago to apply the kinetic theory of gases to atmospheric problems, I became impressed with the weakness of the gaseous hypothesis of the earliest stages of the earth's evolution, and subsequent studies of the relations of mass and momenta, with the essential aid of Dr. Moulton, led to still graver doubts as to the tenability of the Laplacian hypothesis, on which both of the preceding conceptions of the origin and distribution of internal heat are based. This led to studies upon alternative hypotheses. Among these is the conception that the earth, instead of descending from a gaseous spheroid, may have been built up by the gradual ingathering of its material from a scattered meteoroidal or planetesimal condition. If the infall were sufficiently rapid a molten, and even a gaseous, condition would result, but if the infall were slow the surface heat arising from impact might be radiated away practically as fast as generated, and the accretion might proceed with comparatively cool surface temperatures. The source of the obviously high internal temperatures of the earth then arose as a question crucial to the hypothesis. The suggestion that it might be due to gravitative compression arising from the earth's increasing mass was submitted to preliminary computation with favorable results. This therefore appeared to give a third working hypothesis of the origin

and distribution of internal heat. In this case, manifestly, the greatest heat would arise where there was the most compression, and the distribution of internal temperature would have a genetic relation to the distribution of internal pressures. As the second hypothesis also makes pressure a genetic factor in determining the temperature of solidification, these two hypotheses have much in common, so far as thermal distribution is concerned. In their geological bearings they differ somewhat radically from the heretofore more prevalent hypothesis of uniform initial temperature; for, if the heat gradient rises continuously—though not uniformly—to the center, there must be an outward flow of heat at all points, and hence the whole body of the earth must participate in the thermal changes, and in the effects of these on earth movement and deformation.

In these preliminary studies it was observed that the nature of the pressure curve derived from the Laplacian law of density, or from any other probable law, was such as to suggest that there would ensue an internal redistribution of heat of such a nature that the outer portions, neglecting the surface shell, might experience a rise of temperature independently of any surface loss of heat.

These considerations, briefly outlined here to render more intelligible our working scheme, are so fundamental that it has seemed worth while to spend some time in definitely developing the three hypotheses into such specific forms as would secure from them their best working stimulus and their greatest suggestiveness. This I have attempted to do.

The origin and distribution of internal heat being thus fundamental and critical, Mr. A. C. Lunn has undertaken a mathematical inquiry into the thermal effects of compression under the accretion hypothesis. As the data relative to compressibility, conductivity, specific heat, and other factors involved cover only the small range of conditions available in experimentation, and as even the possibilities of experimental determination have been as yet by no means exhausted, either in range or in accuracy, the method of multiple hypotheses has been used to cover the probable range of variation that may arise under interior conditions. The work is also being formulated so that further extensions of application may be easily made if required. While it is to be regretted that laboratory data are not better, there is compensation in the fact that such an inquiry as is now in hand may show in what lines the tedious and expensive work of experimental determination may be directed with the greatest prospect of fruitful results and what lines may be avoided as

likely to prove fruitless. A function of preliminary mathematical inquiry is to discover productive fields, as well as desert tracts, before serious and expensive cultivation is attempted.

More specifically, Mr. Lunn has had under consideration the following problems:

(*a*) The determination of the original distribution of temperature in a solid earth where heat energy is supposed to have arisen from gravitative compression, on various assumptions as to internal density and specific heat. The assumptions most closely related to the Legendre-Laplace law of internal density have been given precedence. The determinations, however, are given such forms that a short computation is sufficient to reduce to numbers the results of any assumed law likely to be entertained at any future time.

(*b*) The determination of the total heat energy due to a merely gravitative contraction of the earth mass, the assumptions in this case corresponding to those under (*a*). The development involves the proof of the consistency of the points of view assumed in (*a*) and (*b*) relative, the one to the distribution of heat, the other to its total amount. Comparison has also been instituted between the chief bodies of the solar system, in these particulars, for the sake of the collateral light they may throw on the earth problem. The immediate inquiry does not contemplate more than first approximations, but it is hoped that it will show the lines along which further and more involved researches may best be attempted, by excluding barren ground and narrowing the range of hopeful inquiry.

(*c*) Postulating the previous determinations of the initial distribution of temperature, a theoretical determination of the history of the cooling of the earth has been attempted, assuming various laws as to conductivity. This has been found to offer very serious difficulties, both in the analysis and construction of formulas, and in the complexity of the computations. Enough has been done, however, to show the probable validity of the conception, derived from preliminary studies, that the initial stages of redistribution of heat involved an actual increase of temperature in regions just below the outer shell, and that this extended over some length of time from the beginning of the process.

(*d*) Some attention has been given to such collateral problems in elasticity as were likely to throw light on the assumptions made in the previous computations, especially those calculated to supplement the meager experimental data. Mr. Lunn has also cooperated in the informal discussion of other themes embraced in our joint studies.

Since experimental data relative to the compressibility of rock must, at the best, always be limited to a rather narrow range, it is important to make such use as is possible of the geological evidence relative to the actual compression that has taken place. For the statistical data bearing on this we must look to geological measurements of the extent of the deformations that have been suffered by the earth. In the interpretation of these the assignable sources of the compressive agencies and their modes of action are important. As one step in this direction, an attempt has been made to test the validity of the suggestion, growing out of the researches of G. H. Darwin, that a change in the rate of rotation of the earth, due to tidal action, has reduced the volume of the earth by compression arising from increased gravity, and has also reduced the surface area by change of form, in addition to the reduction due to lessened volume. As Professor Slichter had previously made computations bearing on this subject, his cooperation was sought, and generously given. As a convenient approximation, a homogeneous earth had been assumed in the previous computations. For a closer approximation, he recomputed the reduction in surface area on the basis of an earth whose interior density changed according to the Laplacian law. For a broader application, the computations were made to include a series of fourteen rotation periods, ranging from 3.82 hours to 23.934 hours. The following factors were computed for each period: polar radius, equatorial radius, ellipticity, equatorial attraction, polar attraction, centripetal acceleration at the equator, latitude of mean radius, equatorial contraction (in percentage and in miles), and meridional contraction (in percentage and in miles).

It will be seen that these data cover a multitude of special cases and may be used in almost any case liable to arise. The mode of application and the general tenor of the results may be illustrated as follows: With a change of rotation from 3.82 hours to the present rate, the equatorial belt must shorten 1,131 miles, while the meridional circles must lengthen 495 miles. If the crust shortening involved in the formation of the Alps be taken at 75 miles, regarded as a very ample if not excessive estimate, the 1,131 miles of equatorial shortening would be sufficient to form 15 mountain ranges of Alpine magnitude. These should run across the equatorial belt and die away at the latitude of mean radius, 32° 22′. The contemporaneous high latitude tension would be sufficient to cause the crust to gap or to stretch in the polar arcs more than 200 miles. The coordinate geological inquiry shows no such remarkable distri-

bution of thrust and tension. Obviously the rocks of earliest formations should show the differential effects of change of rotation most notably, but the Archean formations of high latitudes, where tension should prevail, are crumpled and crushed much the same as in low latitudes, where thrust should prevail. Mountains of all ages are about as abundant north of 33° latitude as in the equatorial belt. Nor are the equatorial mountains notably transverse to the equator, or limited in extent to the demands of the hypothesis. Furthermore, if there had been appreciable change in the form of the earth to accommodate itself to a slower rotation, the water on the surface, being the most mobile element, should have gathered toward the poles, and the less mobile solid earth should have protruded about the equator, but the distribution of land and water, present and past, gives no clear evidence of this. The equatorial belt contains a less percentage of land than the area north of it, and more than that south of it. It varies but slightly from the average for the whole globe.

Geological evidence being thus out of harmony with deductions from the researches of G. H. Darwin on the earth-moon evolution under tidal control, inquiry as to the source of the discrepancy was naturally invited. The influence of the ocean tides is probably ineffectual because the varied positions of the derived tides are such as to nearly neutralize their own effects mutually. This conclusion has also been announced recently by Poincaré.* The essential question, therefore, resides in an assumed body tide, and this brings into sharp emphasis the question whether the earth is not either too rigid to give an effective body tide, or so resilient, owing to its high elasticity, that the tides do not have the right position to be effective in retardation; for retardation of rotation is as much dependent on the carrying forward of the tidal protuberance by the earth's rotation as on the amount of the protuberance.

We have endeavored, therefore, to advance the study to include the distribution of rigidity in the earth, as well as its amount, and to recognize the effects of elasticity on the response and the resilience of the spheroid to the tidal stresses. Tentative laws of distribution of the internal rigidity have been formulated, two of which may serve a temporary purpose until better grounds can be developed. At the present stage of inquiry, it would seem that geo-

* L'influence des marées océaniennes sur la durée du jour est donc tout à fait minime et n'est nullement comparable à l'effet des marées dues à la viscosité et à l'elasticité de la partie solide du globe, effet sur lequel M. Darwin à insisté dans une serie de mémoires du plus haut intérêt. (Bulletin Astronomique, tome XX (June, 1903), p. 223.)

logical evidence favors extremely high rigidity and elasticity, in contradistinction to the viscous, and even fluid, implications so long urged on the basis of certain geological phenomena.

Professor Hoskins has recently computed the effective rigidity of the earth from the periodic variation of latitude, using methods somewhat different from those of Newcomb, Hough, and Woodward, and reaching results in such a form as to be directly applicable to the problem of tidal retardation of the earth's rotation. These results give the basis for a new and promising line of approach to the problem of tidal retardation, for they permit the substitution of a definite rigidity, determined on independent grounds, for the assumed viscosity on which Darwin's classic investigation was founded. Without going into details, it will suffice to indicate the general bearing of the investigation to say that the recomputed rate of retardation is only about one fourth as great as that found by Darwin. In reaching this result the *position* of the tide was assumed to be that most favorable to retardation. The determination of the actual position that the tide would assume with the given rigidity and elasticity has not yet been attempted, and the amount by which it will modify the above result is unknown. If it modifies it at all it will be in the nature of a reduction and will further tend to bring the results of computation more into harmony with the geological evidences. The same line of inquiry promises other results of value relative to the state of the earth's interior.

Previous studies had led to the conviction that the stress accumulating competency of the earth's body affords a promising line of approach to the physical state of its interior, and Professor Hoskins has cooperated in certain preliminary steps intended to test the validity of this conviction and to develop the problem. The general line of reasoning may be briefly indicated. Innumerable gentle warpings have affected nearly every portion of the surface of the globe at nearly all stages of its history. This implies the perpetual activity of minor forces of deformation and a concurrent yielding of the outer part of the earth to these forces. At the same time, when the master phenomena of movement and deformation are considered, there appear to have been long periods of relative quiescence, followed by epochs of profound deformation. This general view, long held by leading geologists, is being greatly strengthened by the working out of the great baselevels, which add evidence of the most cogent kind relative to the quiescent stages, while the stratigraphic evidence of the periodicity of the great deformations is regarded as

ample. As the known fundamental agencies, such as the loss or the redistribution of heat, work constantly rather than periodically, it is inferred that the stresses arising from them are sustained by the strength of the body of the earth until they have accumulated to an intensity sufficient to compel yielding and consequent deformation. The magnitude of the deformations being measurably determinable from geological data, the problem is to determine what states of the interior matter are required to accumulate such stresses, and how large portions of the mass, in these requisite states, must have been involved to meet the requirements of the case. The preliminary tests seem to indicate that at least a large portion of the whole globe must have been involved, and that the effective resistance of this must have been of a high order. These results tally well thus far with those derived from studies on the rigidity of the earth on independent lines. Professor Hoskins's other engagements have not permitted him to seriously attack the more difficult phases of this promising line of inquiry.

The logical connection between atmospheric studies and the foregoing problems may not be evident, but it is quite real. My own special interest in them sprang from climatic problems which led back through the history of the atmosphere to primitive states and fundamental conditions. A special study of a gypsum deposit of Iowa, conducted in part under my supervision, by a fellow of the university, Mr. F. A. Wilder, seemed to the suggestive mind of Dr. Stieglitz to afford a means of testing the atmospheric conditions relative to the critical element, carbon dioxide, at the time of the precipitation of the gypsum, which Dr. Wilder interprets as Permian. At my request, Dr. Stieglitz has undertaken the investigation. It is not yet complete, but its nature may be indicated. The significant feature of the gypsum deposit is its remarkable freedom from calcium carbonate. Since in the evaporation of sea water of the present content of salts and under the present conditions calcium carbonate is precipitated before calcium sulphate (gypsum), and since with a constant supply of fresh sea water calcium carbonate would be precipitated continuously with the gypsum, various hypotheses were suggested to account for the obvious removal of the calcium carbonate from the brine during its evaporation without its being deposited before or with the gypsum of these beds.

The present investigation is intended to apply the laws of equilibrium in salt solutions to this problem. The solubility of calcium carbonate at a given temperature is primarily a function of the

atmospheric content of carbon dioxide and of the concentration of calcium ions in the solution. The solubility of gypsum is a function of the concentrations of calcium ions and sulphate ions. By a comparison of the solubility curves of the two salts as affected by these variable factors, it is possible that a point may be found where the curves intersect in a way which might permit a reversal of the usual order of precipitation. If such a point is found, it would throw light on the condition of the atmosphere and of the surface of the earth at the geological period involved. The necessary calculations will be restricted at present to solutions of the ions in question, and may later be made to include the other ordinary constituents of sea water.

Three of the papers, contemplated as preliminary reports on the foregoing studies, are partially prepared and might be speedily completed were not deliberation and a review of the grounds involved in these complex themes more important than early production.

CHICAGO, *September 14, 1903.*

ARCHEOLOGICAL AND PHYSICO-GEOGRAPHICAL RECONNAISSANCE IN TURKESTAN

REPORT BY RAPHAEL PUMPELLY.

CONTENTS.

At the end of 1902 the Carnegie Institution voted a grant to me "for the purpose of making, during the year 1903, a preliminary examination of the Trans-Caspian region, and of collecting and arranging all available existing information necessary in organizing the further investigation of the past and present physico-geographical conditions and archeological remains of the region."

The investigation was proposed because (1) there is a growing belief that central Asia was the region in which the great civilizations of the far East and of the West had their origins; and (2) because of the supposed occurrence in that region, in prehistoric times, of great changes in climate, resulting in the formation and recession of an extensive Asian Mediterranean, of which the Aral, Caspian, and Black seas are the principal remnants.

It had long seemed to me that a study of Central-Asian archeology would probably yield important evidence in the genealogy of the great civilizations and of at least several of the dominant races, and that a parallel study of the traces of physical changes during Quaternary time might show some coincidence between the phases of social evolution and the changes in environment; further, that it might be possible to correlate the physical and human records and thus furnish a contribution to the time scale of recent geology.

At my request Professor W. M. Davis assumed charge of the physico-geographical part of the preliminary reconnaissance.

ITINERARY.

I left Boston March 18, accompanied by Mr. R. W. Pumpelly as assistant, and stopping over at London, Paris, and Berlin, reached St. Petersburg on April 23. There I had to remain several weeks to perfect arrangements and obtain the papers necessary for an extended journey in Turkestan. On May 15 we left St. Petersburg with an interpreter, and having been joined at Baku by Professor Davis and Mr. E. Huntington, a research assistant of the Carnegie Institution, we crossed the Caspian.

I found throughout our stay in Turkestan that orders had been sent from St. Petersburg to assist the expedition in all ways, and everything was done to facilitate the work. Prince Hilkof's orders obtained for us the continuous use of a car throughout our stay in Turkestan.

While I became deeply indebted to the general hospitality of all with whom we came in contact, I am under especial obligation to several gentlemen to whose ready assistance the expedition owes much of its success. From their excellencies Count Cassini and the Hon. Joseph H. Choate, Assistant Secretary of State Mr. Herbert Pierce, and Baron von Richthofen I received valuable letters to St. Petersburg. There, from His Excellency Mr. Semenof, vice president of the Imperial Geographical Society, I had letters of the first importance to high authorities in Turkestan, as well as from Generals Stubendorf and Artemonof. Valuable assistance was rendered by Mr. McCormick, our ambassador, and Mr. Ridler, secretary of the embassy.

Their Excellencies Prince Hilkof, Minister of Ways and Communications; Mr. Plehve, Minister of the Interior, and Mr. Yermolof, Minister of Agriculture, gave me circular letters to all the employes of their departments; while from the office of the Minister of War, who has control of Turkestan, orders were telegraphed to extend any desired aid to the members of the expedition. My plans were also cordially furthered by the Imperial Academy of Sciences at St. Petersburg, which passed a resolution asking the Minister of the Interior to facilitate our journey; by Mr. Karpinsky, then director of the Imperial Geological Survey; Professor Schmidt, and Mr. Bog-

danovitch, and by Mr. Chernachef, now director of the Imperial Russian Geological Survey.

In Turkestan we enjoyed the hospitality and assistance of their Excellencies the Governor General and Madame Ivanof; General Medinsky, governor of Samarkand; General Nalifkin, vice governor of Fergana, and Madame Nalifkin; General Oussakovsky, governor of Trans Caspia; Colonel and Madame Volkovnikof, local governor of Krasnovodsk; Colonel Kukol-Yasnopolski, governor of Ashkabad; General Ulianin, director of the Trans-Caspian railway; General Poslovsky, and General Gedienof. I owe the success of our Pamir expedition chiefly to the active interest and help of Colonel Zaitza, governor of Osh. To Baron Cherkasof, political agent at Bokhara, I owe much for his kindness during my visit to that place. At Old Merv we were entertained with great hospitality by Mr. Dubosof, superintendent of the imperial estate.

Using the railroad as a base and having horses and escorts wherever needed, we made flying excursions to many points, at different distances from the railroad, both in going and coming.

From Ashkabad we made an excursion across the mountains of Khorassan into Persia, accompanied by Mr. Yanchevetzki, secretary of the governor, and his intimate acquaintance with the water problems and with the country from the Aral south was of great use to us. On our return to Ashkabad we were joined by Mr. Richard Norton, who accompanied me throughout the journey.

The next stop was at Old Merv, where we spent several days among the extensive mines. Thence passing by Bokhara and making only a preliminary visit to Samarkand, we went to Tashkend, the residence of the governor general of Turkestan. Here the party divided, Professor Davis and Mr. Huntington going eastward to Issikul, where, after a month of joint work, they separated, Mr. Davis returning to America via Omsk and St. Petersburg, and Mr. Huntington going on to Kashgar.

After Tashkend, I visited Margelan and Andijan, the end of the railroad.

Continuing our journey to Osh, at the entrance to the mountain region, we organized an expedition to the Pamir, with the courteous aid of its governor, Colonel Zaitza. The way to the Pamir covered part of the route and two of the passes, the Terek and Taldik, in one of the great currents of ancient trade between China and western Asia, and it promised light on the physico-geographical part of our problem. After returning from the Pamir we visited the ruins

of Ak-si, in the northern part of Khokand, beyond the Sirdaria and examined the ruined sites of Samarkand, and of Paikend in Bokhara and a trenched tumulus at Annau near Ashkabad.

Throughout the journey, both by rail and in the side excursions, we had occasion to note the existence and position of a great number of former sites of occupation, both towns and tumuli.

It had been my wish to examine Balkh, the site of ancient Bactra, and other ruins of northern Afghanistan, but this was found to be impossible on account of the hostile attitude of the Afghans toward even Russians.

Outline Sketch of the Region.

A glance at a map of the Eurasian continent shows that the three seas, the Aral, Caspian, and Black, occupy parts of one great basin, bounded on the south and east by great mountains, and on the north by the Aral-Arctic divide.

If the Bosphorus were closed and there should exist a continued excess of rainfall over evaporation, these seas would merge and the basin would fill till it overflowed into the Northern ocean. The area of this Asian Mediterranean would be determined by the height of the northern divide, which is as yet unknown. In any event, it would be sufficient to submerge a large part of southern Russia and much of Russian Turkestan.

If, on the other hand, there should be a continued increase of excess of evaporation, the seas would dry up; the whole basin would be transformed into a vast desert, on the borders of which the retreating river mouths would be lost in the sands. Turkestan, once largely covered by water, is now in a state approaching this condition of aridity. The greater basin is broken up into smaller, disconnected ones, of which only one, the Black sea, has an outlet. The Aral stands 159 feet above the ocean, the Black sea practically at ocean level, the Caspian 84 feet below ocean level.

The great Volga and several small streams reach the Caspian; east of the Caspian only two rivers, the Sir and Amu (Jaxartes and Oxus), reach the Aral; and they gather water only at their sources in snow clad mountains; all other streams are consumed by direct evaporation and irrigation and have short courses, ending in desert sands.

According to Schwartz, about three quarters of all this vast region is desert and one quarter is capable of supporting the herds of the

nomads. Water can be distributed on about 2 per cent of the entire area, on land free from drifted sands. Along the base of the southern mountains stretches a chain of narrow oases at the mouths of the mountain valleys; there are other very narrow strips along the larger river courses, and more extensive areas inclosed between the projecting spurs of the eastern mountains; all the rest of the basin has become the prey of the moving sands, which are still very slowly but surely invading the oases. The boundary is sharply defined; within it is high cultivation; beyond is a sea of waves of sand.

As they extend eastward the southern mountains increase in height, till both they and the great spurs of the Tienshan—giant snow and ice covered crests and peaks—dominate the oases which are the offspring of their waters. It is on this mountain snow and ice that the life of the whole region is and has been from a remote period absolutely dependent.

This life is also limited by another factor—itself a result of the desiccation—the moving sands. For, other things remaining equal, while the shrinkage of the water areas can continue only till equilibrium between supply and evaporation is reached, and while there might be also cyclical periods of revivifying afflux, these compensations are offset in the oases by the steadily overwhelming progress of the sands.

The progressive desiccation of Turkestan is shown by direct observations during the past century, by artificial landmarks, by historical statements, and by natural records. The Aibughir gulf of the Aral was 133 kilometers long and 3,500 square kilometers in area in 1842, and dry land in 1872.

The volume of the Sirdaria has diminished greatly, as shown by the remains of old irrigating canals along its whole lower course, which are now too high to receive water. The statements of Arabian writers show that, within recent historical times, there was a far more numerous population than the country could support now, when all available water is utilized. Old water level lines occur at various heights up to 225 feet above the Aral.

The progress is not uniform, but is broken by periods of temporarily increased precipitation. Dorandt measured in 1874–75 a fall of 70 millimeters in the year in the Aral sea. Schultz, in comparing his surveys of 1880 with earlier maps, found a lowering of the level of 38 centimeters in nine years. On the other hand, Berg in 1901, comparing the gage established by Tillo, found the level 121

centimeters higher than in 1874. He calculates the total rise between 1882 and 1901 to be at least 3 meters, or 178 millimeters, yearly.

Judging from my observations and from those of others, especially of the Arabian writers and of the later Russian explorers, it would seem that the country has long been an interior region, dependent mainly on the snows and glaciers of the mountains for its life; that there have been within the present geological period great fluctuations in the amount of water derived from the mountains as recorded in high and low shore lines of the seas, and in the strata left by different expansions of the united waters of the Aral and Caspian and containing living forms; that man already existed within the region during at least the last great maximum of moisture.

Evidences of Former Occupation.

In our earliest historical records we find the country occupied as now by dwellers in numerous cities, surrounded by deserts in which lived nomad peoples. The town dwellers seem to have been at least largely of Aryan stock and the nomads of Turanian.

Who were the contemporaneous and the successive dwellers in the many towns? To what different races may they have belonged? Whence did they come into the land? What were their civilizations and what their relations to other civilizations and to those of the modern world? These are our questions, and they can be answered only to a greater or less extent by a study of the results of excavation and in the concentrated light of comparative science in archeology, ethnology, and language and of survivals in arts and customs, for the answers to some of these questions will be found rooted deep in the human strata of the ancient world. Asia abounds in the fragmentary survivals of stocks, arts, customs, and languages.

The vestiges of former occupation by man are varied in character—in the eastern mountains pictographic inscriptions recalling those of American aborigines, some rock sculpturing, and rough stone idols. At Lake Issikul Professor Davis describes stone circles, recalling some of the dolmen-like forms, and submerged masonry in the lake.

Along the river courses are abandoned canals which can no longer be supplied with water, and the Russian maps abound in indications of ruined towns, "forts," etc. The most important remains are the tumuli and the town sites.

TUMULI (OR KURGANS).

The tumuli proper are accumulations of earth, of rounded generally symmetrical form, often more or less elliptical in horizontal section. We met with them first along the base of the mountains east of the Caspian, but I saw none at a lower elevation than 250 feet above that sea. From this point eastward they abounded, with some interruptions, as far as to near Andijan. Generally they were large—100 to 200 feet long and 30 to 50 feet high. They are much more abundant east of the Oxus than to the west. At one point I counted fifteen in sight at once. Besides these larger tumuli, there are, especially along the Sirdaria in Fergana, localities with a great number of small mounds a few yards only in diameter, suggesting burial after battles.

Mounds more or less resembling the larger ones are described by De Morgan at points in northern Persia, and they occur through southern Siberia and on the plains of southern Russia and of Hungary. In all these countries they probably have different origins—different reasons for their existence. Those in Siberia and on the Black sea have been extensively excavated.

There has been some unsatisfactory excavation of those in Turkestan, mostly with unrecorded results. The kurgan at Annau, near Ashkabad, was trenched some years ago by General Komorof. This afforded the best exposure of internal structure. It is nearly 200 feet long and 35 feet high and slightly elliptical in horizontal section. It consists of fine, horizontally stratified layers of made earth. Layers of silt and broken cobbles alternate with layers rich in gray ashes and charcoal, and others of closely matted fragments of pottery. Animal bones, teeth, and jaws, some of which are partially calcined, occur frequently in all layers, with a few human bones and skulls. Several whole vases and muffle shaped chests, made of coarse pottery mixed with dung, had been cut by the trench. These appeared to contain only fine ashes and charcoal. Most of the fragmentary pottery is of this coarse, dung mixed quality, but there are also many fragments of finer texture, decorated with sample designs of black on red, even at the bottom of the trench. We found several granite stones with curved plane surface which had evidently been shaped for mealing grain by the *metate* method, and also a roughly spherical stone that had been pierced, apparently for the insertion of a handle and to use as a maul. Gen-

eral Komorof found one celt of quartzite and some needles of bone, but absolutely no metal. Of the bones, I sent a representative collection to Professor Zittel in Munich, for determination.

The whole character of the tumulus shows that it grew from the plain upwards, as a slow accumulation of the debris of long occupation. The fact that the layers, even at the top, extend horizontally to the edges proves that it was formerly flat topped and much larger, for had it during occupation ever assumed a spherical surface the growth would have been in concentric layers. The same reasoning would show that it was never abandoned for a long time and again occupied. Since its surface has not been gullied, it seems possible that it was shaped by wind action, although the earth is somewhat firmly cemented. A further indication of antiquity is the present condition of the granite grain grinders, now rotten and crumbling.

One peculiar feature in the structure is the interruption and bending over of the layers at the two apparent earth walls.

Several other kurgans that we examined, which had been partially cut away for brick making, etc., and some of which were much larger and higher, showed the same horizontal stratification of earth, burnt earth, ashes, charcoal, and fragments of bones and of pottery. In the upper part of some of these we observed traces of walls of unburned bricks. The only artifacts found in these were the simplest form of flat stone for grinding grain (like those found in the Annau kurgan) and some flat stones, each with a hole drilled wholly or partially through it from both sides.

Ancient Towns.

The absence of easily obtainable stone throughout the lowlands of Turkestan determined the use, almost exclusively, of clay, both unburned and burned, in construction. Unburned clay predominated immensely, used both as sun dried bricks and in heavy layers of raw clay. In consequence of this, all ruins older than a late Mussulman period are represented only by accumulations of earth filled with broken pottery and fragments of burned bricks. These accumulations are flat topped mounds, ranging up to a square mile or more in area and from 15 to 20 feet upward in height, and in places, as at Merv, occurring in groups covering many square miles. They occur within areas in which now, or formerly, water was accessible, and are found also more or less buried in sands beyond

the mouths of the retreating rivers, in places once fertile and now desolate.

Ruins near Atrek River.—A type of regional desolation and abandonment is in the territory between the lower Atrek and the Caspian. Here, over an area of many square miles, are the ruins of cities, 30 or 40 miles from the river Atrek, the nearest water, and in the heart of the desert. The remains of canals show that the cities were watered from the Atrek, but this river now lies too low to feed the canals.

Ancient Merv.—The ruins of ancient Merv are said to cover about 30 square miles and consist of several cities of different ages. Two of these—the Ghiaour Kala and the Iskender Kala appear to be the more ancient. The remains of a circular wall extend, with a radius of about four miles, all around these several cities. To judge from its degraded condition, it may possibly represent a very ancient enclosure within which diminishing populations have rebuilt after successive destructions by war. Merv existed in remote antiquity and is one of the cities mentioned in the Zend Avesta.

The walls of Ghiaour Kala, though now reduced to a hillocky ridge perhaps 50 or 60 feet high, enclose plateaux, 20 or 30 or more feet high, of accumulated debris. From these walls we could see far away on the northern horizon, in the desert, other flat topped mounds apparently of great height and extent.

Ruins of Paikend.—The ruins of Paikend represent the type of cities abandoned for lack of water and then buried by the progressing desert sands. It was a great center of wealth and of commerce between China and the west and south till in the early centuries of our era. The recession of the lower ends of the Zeraffshan river brought its doom. Now only its citadel mound and the top of parts of its wall rise above the waves of the invading sands.

Samarkand.—Next to those of Merv the ruins of Samarkand are the most extensive. Its position must have made it an important center of commerce and wealth probably throughout the whole period of prehistoric occupation, as it has been during historic times. Situated in the heart of the very fertile oasis of the Zeraffshan river, it lies also on the most open and easiest caravan routes connecting China and eastern Turkestan with Afghanistan, India, and Persia.

Samarkand has, even within the past two thousand years, been sacked, destroyed, and rebuilt many times. Like Merv, its rebuildings have often been on adjoining sites, and the determining of the whole area covered by these various sites remains to be made. There

is evidence that it is very extensive. The most ancient seems to be the plateau or "tell" called "Afrosiab," to which tradition assigns the site of the Samarkand Maracanda of Alexander the Great. This is a plateau of "made earth," the debris of ruins, standing on the "loess" plain. It is covered to a great extent with Mohammedan cemeteries, and some traces of Mussulman occupation, and with fragments of pottery and of bricks. The loess plain is deeply dissected by a stream, and several gullies have been cut in both the plateau of the ruins and the loess. It is difficult to distinguish between the "made earth" of the plateau and the underlying "loess," except through the presence of fragments of pottery, charcoal, and bones.

We found such fragments down to a depth of about 40 feet below the general surface, in the gullies, and it is not improbable that the thickness of debris is still greater. Above this general surface rises the citadel mound to an additional height of 30 to 40 feet, or 170 feet above the stream at its base. Judging from the excellent topographical map of Afrosiab, of the general staff, the loess plain lies about 50 feet above the stream. This would make it possible that the citadel mound represents an accumulation of over 100 feet of debris. The surface of the rest of Afrosiab is very irregular. While in general it ranges from 100 to 140 feet above the stream, there are numerous depressions, the bottoms of which are level plains, 150 to 300 feet in diameter, standing 70 to 80 feet above the stream.

The general arrangement of these depressions is such that if filled with water they would form a connected, irregular system of ponds; and there is a channel about 100 feet wide which starts in high up on the cliffs overhanging the stream, and, traversing Afrosiab, opens out again on to the stream valley, after communicating with most of the depressions. It all suggests a former water-system, but it is one that it would seem could have been effective only if the stream ran at a considerably higher level than at present. The large scale map of the district shows this stream to diverge from the Serafschan in the same manner as many irrigating canals, and to run with a lower grade than the parent river, the river having a grade of about 20 feet per verst as against about 7 feet in the derivative stream or canal. Judging from these facts it seems not impossible that this stream was originally a canal supplying the city, and that it has in the course of ages cut its channel deeper in the "loess."

The former walls of the city are represented now by ridges rising

20 or 30 feet above the surface within. Where the walls are cut by gullies old galleries are exposed which seem to have been continuous with the wall. Quintus Curtius states 70 stadia as the extent of the walls in the time of Alexander. This, if the short stadia were meant, would be about three miles, which would be approximately the circumference of that part of Samarkand now called Afrosiab.

As in all Turkestan, so at Samarkand, the older structures still standing are those of the Mohammedan period. The many immense and wonderfully decorated mosques built by Tamerlane, though now falling into ruin, belong among the wonders of the world; and this not only on account of their great size, but also because of the beauty of their decoration. Seen from Afrosiab, these ruins tower high above the rich foliage of the oasis city—evidences of the wealth of treasure that Tamerlane had accumulated in Turkestan within two centuries after Genghis Khan had sacked the country and massacred much of its population.

Review of the Field.

What I have been able to say here regarding the archeology of Russian Turkestan seems but a meager statement; but it was soon clear that all that could be accomplished in such a reconnaissance would be the observation of the character and abundance of the evidences of former occupation, and to obtain some idea of their distribution and size.

Our reconnaissances covered a territory nearly 1,400 miles long. It was necessarily only of a preliminary character, and intended to supply a general idea of the problems to be solved and of the best points at which to begin.

While we have been surprised at the abundance of the data offered by the region toward these solutions in natural and artificial records, we are impressed with a realization of the intimate relation in which this region stands to the Quaternary and prehistoric history of the whole continent. Physically it forms part of the great interior region extending from the Mediterranean to Manchuria, whose history has been one of progressive desiccation, but in Russian Turkestan the effects of this have been mitigated by the snows of the lofty ranges and the lower altitude of the plains.

Archeologically this region has, through a long period, been a center of production and commerce, connecting the eastern, western,

and southern nations, and accumulating wealth that has made it repeatedly the prey of invading armies. It has been from remote time the field of contact and contest between the Turanian and Aryan stocks; but its problems, both physical and archeological, are parts of the greater problem underlying the study of the development of man and his civilization on the great continent and of the environment conditioning that development.

The many fragmentary peoples surviving in the remote corners and in the protected mountain fastnesses of Asia, preserving different languages, arts, and customs, indicate a very remote period of racial differentiation, with subsequent long periods for separate development. They point also to the long periods of unrest and battling in which the survivors of the vanquished were forced into their present refuges. And this unrest was probably the remote prototype of that which in later prehistoric and historic time sent out its waves from the Aralo-Caspian basin. It was probably from the beginning a condition in which the slowly progressive change toward aridity in interior Asia was ever forcing emigration outward, displacing other peoples, and thus working against the establishment of a stable equilibrium of population.

Asia is thus the field for applying all the comparative sciences that relate to the history of man. The materials lie in cave deposits, in rock pictographs, in tumuli, dolmens, and ruined towns, in languages, customs, religions, design patterns, and anthropological measurements.

Turkestan, from its geographical position, must have been the stage on which the drama of Asiatic life was epitomized through all these ages of ferment. Races and civilizations appeared and disappeared, leaving their records buried in ashes and earth; but the fertility of the soil produced wealth, and the position kept it ever a commercial center.

So far as our problems of archeology and physical geography are concerned, Turkestan is practically a virgin field. In geology and cartography the Russians have done a surprising amount of excellent work; but the modern methods of physico-geographic study have not been applied, and the little archeological work done has been in the nature of hunting for curios and treasure. Throughout southern Siberia tumuli have yielded up vast treasures in the form of gold ornaments dating from various epochs. Scientific excavation has been undertaken only in southern Russia, in the Caucasus, and in Persia.

In Persia, M. J. de Morgan has for several years been conducting a thoroughly scientific investigation at several points, and especially at Susa, where he has already obtained results of the greatest interest. The acropolis of Susa is 105 feet high. M. de Morgan's preliminary tunnels, run into the hill at different levels, showed it to be composed of made earth from the base upward. Stone implements and pottery abounded up to 36 feet from the top. The pottery improved from below up, and among the fragments he recognized a variety belonging to a group peculiar to Egypt, Syria, Cyprus, and most of Asia Minor, but not known from Mesopotamia. De Morgan had found this in predynastic tombs in Egypt, and ascribed it to a period before the eightieth century B. C. At 45 feet below the top he found tablets and cylinders with hieroglyphic inscriptions which Scheil considers as belonging before the fortieth century B. C.

M. de Morgan asks: "If the refined civilizations of the past 6,000 years, with their great structures and fortifications, have left only 45 feet of debris, how many centuries must it have required to accumulate the lower 60 feet when man used more simple materials in the construction of his abodes?"

East of Russian Turkestan excavations have been recently made by Stein in some cities in the Tarim basin, which we know from Chinese history were buried by sand in the early centuries of our era.

The thickness of made earth in the tumuli and town sites of Turkestan is sufficient to give reason for expecting evidences of very long continued occupation. The dryness of the climate makes possible the preservation of any traces of written or incised documents that may have existed. Excavation conducted with the idea that everything met with—the earth itself, the character, position, and association of fragments—is part of history, cannot fail to be most fruitful in results.

It was in all probability from Turkestan that the earliest products of metallurgy in bronze and iron successively progressed to the western world—a progress that in each case carried with it a revolution in civilizations. We do not know whether this region saw the birth of the metallurgy of those elemental substances which, beginning with copper and tin and progressing through bronze to iron and steel and the use of coal, marks the birth of civilization and its great revolutions. If it was not the birthplace of this art, and if it was a distributing center, it is a long step nearer to the source, whether this was China, East Turkestan, or India.

RECOMMENDATIONS.

Since Turkestan is under the control of the Minister of War and much of its frontier is closed to travelers, it is necessary to have the permission and good will of the government in order to pursue investigations. To inaugurate any extensive plan of archeological excavations will require tactful negotiation at St. Petersburg. I have good reasons for believing that the desired concessions can be had on a basis of division of objects found, and with a sufficient time allowance for the study of all the material. Such a plan should include both town sites and large and small tumuli. Of the town sites I would recommend, as points to begin on, in the order stated.

Town sites.—Afrosiab (Samarkand), Ghiaour Kala (Old Merv), Paikend (west of Bokhara), Aksi (on the Sirdaria). The high ruins seen several miles to the north of Ghiaour Kala. A very high one seen from the railroad a few miles west of the Aum-daria.

Tumuli.—Both the tumulus mentioned at Annau, near Ashkabad, and another lying a short distance from it. Others west of Ashkabad, north of Old Merv, near Djizak; also many of the mounds of small size which seem to have a different significance.

As bearing on the age of the tumuli, it is important that the relation of the base of the mound to the surrounding earth be studied to determine by how much, if any, the level of the plain has been built up since the first occupation of the site, and to see also by how much the mound has shrunken in size at its base, as it certainly has n horizontal section at the top. In connection with the question of age of the tumuli and in relation to the last expansion of the Aralo-Caspian seas, it would be very desirable to determine the lower altitude limit of distribution. I did not see any below 250 feet above the Caspian.

Similar observations are needed on the west coast of the Caspian, where De Morgan found no antiquities on the lowlands in the Lenkoran country, but at a higher level abundant tombs of the bronze period and of the transition to iron.

As further connected with the relation of human occupation to the formerly expanded water area, there is needed a determination of the altitudes of the Manytsch divide between the Caspian and the Black sea, and of that between the Aral and the Arctic ocean. Both of these are now not far from railroad bases.

RESULTS IN PHYSICAL GEOGRAPHY.

Both our own observations and the excellent and extensive work of the Russian geologists show that the progressive desiccation of the region has greatly diminished both the area of cultivable land and the volume of water, and greatly reduced the population. Is this change a phase of cyclical phenomena—of cycles of long periodicity? In what relation have the geologically recent secular phenomena in central Asia stood to man and civilization in that region and to the outside world?

One of the chief objects of the reconnaissance of the past season was to determine whether a systematic investigation would be likely to throw light on these questions. Perhaps the most important result is our finding that successive physical events have left such abundant records, written in large strokes, all over the mountains and the plains.

The work of this year has not only made a most promising beginning in this interpretation, but has shown that it is probably possible to correlate the different events among themselves and with the period of human occupation and possibly with similar physical events in Europe.

As an interior region, central Asia is arid and dependent for its water almost wholly on its bordering mountains. It is also self contained—*i. e.*, without drainage to the ocean. Changes of climate, resulting in great fluctuations of water supply, would therefore probably be recorded by old shorelines at different levels. They might also be more or less legibly recorded in the evidences of repeated glaciation and erosion in the high mountains.

Professor Davis has found traces of an old shoreline about 600 feet above the west shore of the Caspian sea, and a very distinctly marked one on the east side, at an elevation of 200 feet or more. Further search for shorelines was left to form the object of a more extended special study than could be made in our general reconnaissance.

In the Eastern mountains, near Issikul and Sonkul, Professor Davis found clear evidence of two and probably three glacial epochs. Mr. Ellsworth Huntington, working in Kashgaria, found proof of three epochs, and later, of five in the successive moraines of a large number of glaciers studied by him in the Alai mountains. Between some at least of these there were long interglacial intervals.

Mr. Huntington reports records of climatic oscillations shown, not only in these moraines, but also in the valley terraces and erosions, and considers them members of a group of sympathetic glacial phenomena.

Professor Davis noted along the northern edge of the Kopet-dagh, the mountains bordering the plains east of the Caspian sea, and in the Eastern mountains, evidence of a longitudinal dislocation, accompanied by great block uplifts, formed apparently after the wearing down of the mountain masses to a peneplain and preceding an active dissection of the elevated mass. This dislocation had been already observed by Muschketof, who states that it extends far along the edge of the Kopet-dagh.

These block uplifts, by lowering the baselevel, caused a remodeling of the mountains, and have left their record on the lowland plains, which they have helped to create, by the vast amount of material poured out on them by the eroding streams.

The block uplifting and the tilting being correlated with the growth of the alluvial Ferghana lowlands, and the relation of the glacial expansions to the valley cuttings in the Trans-Alai range being clearly recorded, it becomes a matter of great interest to correlate these Quaternary events of the Trans-Alai valleys with those of the Alai, and the growth of the plains with the progress of human occupation.

Mr. R. W. Pumpelly studied independently a profile from the Sirdaria southward across the two mighty snow and ice ranges, the Alai and Trans-Alai. He found clear evidence of two long separated glacial epochs recorded in extensive moraines and on the Pamir in apparently corresponding high shorelines around Lake Karakul. These glacial epochs he has correlated with orogenic movements of the Trans-Alai, there being a definite relation between the glacial trough bottoms of the two epochs and the present stream floors. Of the Alai range, he found that there had been a block uplift followed by a block tilt, both with a dislocation through the border of the lowland plains to the north, and leaving their records in alluvium capped hills and terraces along the valley sides and in the dragging up or tilting of the fluvial sediments or river "fans" on the lowland borders. These movements he has correlated with the glacial geology, making the block tilt an interglacial event.

It is not impossible that, by extending the study of glacial records from the Central Asian ranges through the Elburg and Caucasus, it may be practicable to correlate Asiatic and Alpine glacial events;

and since the great basin was fed both by glaciers of the southern ranges and by the great ice cap of Russia, a correlation of both might be effected; for, in view of the great orogenic movements to which the Caucasus, the Persian, and the Tienshan have been subjected, it cannot be positively asserted that the Central Asian glacial expansions were all contemporaneous with phases of the mundane glacial epoch.

As regards further work in physical geography, Professor Davis writes:

"The order in which I should like to see the * * * studies taken up, in order to most rapidly define the conditions of early human history, on the plains is as follows:

"The shore lines of the Caspian and Aral seas; first on the southwest, south, and southeast, then on the northeast and the associated plains.

"The double belt of piedmont plains and bordering ranges with special work in certain glaciated valleys.

"The deposits of loess from Samarkand to Tashkend.

"The Issikul basin. By a special, independent party.

"Secondarily, Block mountains and the Narin formation."

SUMMARY.

We have shown that the recent physical history of the region is legibly recorded in glacial sculpture and moraines, in orogenic movements in valley cutting and terracings, in lake expansions, and in the building up of the plains, and we have made some progress in correlating these events.

We have also found full confirmation of the statements as to a progressive desiccation of the region of long standing, which has from a remote period continually converted cultivable lands into deserts and buried cities in sands.

We have found, widely distributed, great and small abandoned sites of human occupation, with evidences of great antiquity.

We have reason to think that a correlation of these physical and human events may be obtained through continuance of the investigation, and that archeological excavations will throw light on the origins of Western and Eastern nations and civilization.

APPENDIX

Estimates Submitted by Advisory Committee on Anthropology

The estimates submitted by the Advisory Committee on Anthropology were omitted from the report printed in Year Book No. 1, pages 174 to 181. To make the report complete they are here given:

I. Physical Anthropology:		
I. Purchase of apparatus	$1,500	
II. Annual expenses:		
Salaries of two specialists	3,600	
Two observers, at $600	1,200	
Four computers, at $600	2,400	
Stationery, transportation of instruments, postage, etc.	300	
		$9,000
II. Archeology:		
Salary of specialist	$1,800	
Employment of field assistants and labor	1,500	
Traveling expenses	1,500	
Identification of fossil remains	500	
		5,300
III. Ethnology:		
Salary of specialist	1,500	
Purchase of specimens and traveling expenses, clerical work, etc	3,000	
		4,500
Salary of specialist	$1,200	
Field work	800	
Publication	800	
		2,800
IV. Publication:		
American Anthropologist		1,500
Total grants suggested		$23,100

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